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The Psychological Review

THE PLACE OF THE CONDITIONED-REFLEX IN PSYCHOLOGY[1]

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Since the publication two years ago of my somewhat impolite papers against current methods in psychology I have felt it incumbent upon me before making further unpleasant remarks to suggest some method which we might *begin* to use in place of introspection. I have found, as you easily might have predicted, that it is one thing to condemn a long-established method, but quite another thing to suggest anything in its place. I wish in my remarks tonight to report what progress has been made in this direction.

Probably the first question you will insist upon my answering is: "Why try to find a substitute for introspection? It is a pretty good method after all and has served us well." Rather than stop at this tempting place to enter into a controversy, I shall call your attention to the naturalness of such a quest on the part of the students of animal psychology. The truth of the matter is that animal psychologists have become somewhat intoxicated with success. Finding that an amœba will orient more quickly to certain rays of light than to others, and that a blind, anosmic rat can learn to thread its way through a maze, they begin to look at man with a covetous eye: "After all," they argue, "man is an animal; he moves in response to stimuli in his environment, or to the stimuli offered by the displacement of tissue within his own body. Furthermore, he moves in

[1] Address of the President, before the American Psychological Association, Chicago Meeting, December, 1915.

characteristic ways. Why cannot we study his behavior in the same way that we study the behavior of other animals, modifying our methods to suit this new genus?"

We all admit that many problems in the two fields are similar if not identical. This is especially true of sensory problems. All of us alike wish to determine the various groups of stimuli to which our human or infra-human organism will respond; the various amounts of stimulation necessary to produce these responses, and the bodily areas upon which stimuli must impinge in order to be effective.

Now the animal psychologist has met with a certain degree of success in answering such questions. When we contrast animal psychology in 1900 with animal psychology in 1915 we are forced to admire the enormous strides which have been made in defining problems, in evaluating methods, and in refining apparatus. In 1900 we were content to study by crude methods the elementary features of habit formation in a few easily handled vertebrates. 1916 finds us prepared to carry out on animals as low in the scale as the worm far more delicately controlled experiments than were dreamed of in 1900. The present time likewise finds us prepared to undertake upon the higher vertebrates problems in behavior which in 1900 could hardly have been formulated in behavior terminology. In 1900 who thought of comparing visual acuity in different animals by the use of methods as delicate as those we use on the human being? Or who was bold enough then to assert that in a few years' time we should be using methods for studying vision, audition, and habit formation which are more refined than those which have been employed in the study of the human subject? We must admit, I think, that in the infra-human realm, at least, these years of constant effort have given the animal psychologist a right to look with yearning eyes at this proud genus *Homo*, the representatives of which he finds roaming everywhere, eating any kind of food and from almost any hand, and so resistant to climatic changes that only the lightest kind of covering is necessary to keep them in good condition.

Such in part are the motives which have led the animal

behaviorist to push into gatherings to which he has not been especially invited. Whether we should condemn his enterprising spirit or accept him depends upon how he behaves after admittance. If he can justify his position by deeds, I believe he will be accepted, while possibly not to complete fellowship, at least as an individual who will not bring discredit upon his fellow scientists.

The behaviorist, while meeting no theoretical difficulties in his attempts thus to universalize his methods, does, at the very outset of his studies upon man, meet with very practical ones. In sensory problems when we ask such simple questions as, what is the smallest vibration difference between two tones that will serve as a stimulus to reaction in this particular man, or whether sweet and bitter can be reacted to differently by him, we find that there is no objective method ready at hand for answering them. We know how to employ objective methods in answering such questions with animals. But the animal methods are admittedly slow, and, from the standpoint of the human subject, cumbersome. Some years ago I suggested that we ought to begin to use human subjects in our so-called "discrimination boxes." As might have been surmised, no one took my advice. This was due in part at least to man's upright position, his size, and, I might add, his general unwillingness to work under the conditions which must be maintained in animal experimentation. One can scarcely blame the human subject for objecting to being kept for long stretches of time in a home box the door to which opens from time to time permitting him to pass to the right or left of a partition, and ultimately to reach one or the other of two differently colored surfaces below which he finds a food trough. That which makes the situation still more humiliating to him is the fact that if he has "backed" the wrong color he receives a stone in the guise of an electric shock, in place of the bread which he seeks.

I suggested this rather hopeless method of investigating the sensory side of human psychology because of the increasing desire on the part of many psychologists to see psychology begin to break away from the traditions which have held her

bound hand and foot from the establishment of the first psychological laboratory. I believe that the time is here when the most conservative psychologists are willing to give a lenient hearing to even crude experimentation along lines which may possibly yield an objective approach to sensory problems. This belief has emboldened me to describe briefly our work at Hopkins upon the *conditioned reflex*.

Conditioned Reflexes

In discussing the subject of conditioned reflexes it is customary to make a distinction between (*A*) *conditioned secretion reflexes* and (*B*) *conditioned motor reflexes*. Whether there is any genuine distinction between the two types depends, I think, upon what ultimately will be found to be true about the *modus operandi* of the glands (*i. e.*, whether under such conditions muscular activity is essential to glandular activity or whether control of the glands can be attained independently of the muscles through nervous mechanisms).

A. Conditioned Secretion Reflexes

Before taking up the conditioned motor reflex, with which I am most familiar, I wish briefly to call your attention to one of the most widely known conditioned secretion reflexes, viz., the salivary. The conditioned salivary reflex is well known in this country, thanks to the summaries of the researches in Pawlow's laboratory made by Yerkes and Morgulis, and more recently by Morgulis alone. In brief, this method, which has been under experimental control for some eighteen years, depends upon the following fact: If food (or some similar salivating agent) which produces a direct salivary reflex, and, *e. g.*, a flash of light, are offered jointly for a number of times, the light alone will come finally to call out the salivary secretion. To bring this 'reflex' under control it is necessary to fix upon some method for observing the flow of saliva. This is accomplished usually by first making a salivary fistula, and later attaching a glass funnel to the opening of the duct of the gland. The total flow of saliva may then be measured directly or the individual drops

registered graphically. The use of food for arousing the direct flow of saliva has proved to be slow and not very satisfactory. Most of the work has been done by using acid (dilute HCl). The acid produces a salivary flow immediately and with great sureness.

The conditioned salivary reflex has at present no very wide sphere of usefulness or applicability. In the first place it can be used upon but few animals. Up to the present time it has been used largely upon dogs. Even when used upon these animals the method has very serious limitations. The use of acid for any appreciable time produces stomatitis, according to Burmakin. This makes it almost impossible to carry out investigations which extend over long periods of time. Unless some strong saliva-producing agent is used, the reflex quickly disappears and cannot easily be reinforced. In its present form the method (which calls for operative treatment of the subject) can not be used, of course, on man. Dr. Lashley has been making some tests looking towards an extension of the method. He is experimenting with a small disc grooved on one surface, so as to form two concentric but non-communicating chambers (Fig. 1). The outer chamber, by means of a slender tube, communicates with a vacuum

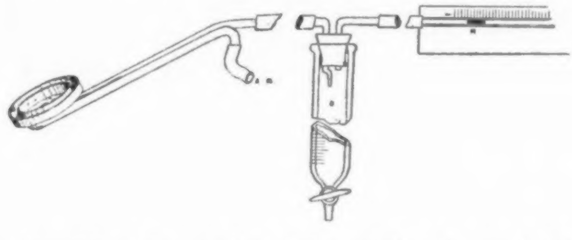


Fig. 1. Apparatus for measuring salivary secretion in man (devised by K. S. Lashley). *o*, outer chamber connecting with vacuum pump, through tube at *AH*. When a vacuum is created the disc clings to the inner surface of the cheek. *i*, inner chamber which is placed over opening of parotid gland. Saliva flows into graduated flask *G* where the total flow of saliva can be measured. Another system of measurement is offered by reason of the fact that when a drop of saliva falls into *G*, air is forced out through a second opening in the flask. When a slender glass tube containing a drop of mercury is attached to this opening, the mercury drop is forced forward a short distance at each drop of saliva. A suitable scale placed behind the glass rod enables one to read and record the number of drops of saliva which fall during any part of the total reaction. (*Sialometer* is an appropriate name for this instrument.)

pump. When the air is exhausted the disc will cling to the inner surface of the cheek. The inner chamber, which is placed directly over the opening of the salivary gland, is likewise supplied with a slender tube which passes out through the mouth. The saliva passing out through this tube can be recorded in different ways. It is too early to make any predictions concerning the usefulness of such a method.

Of the possibility of extending investigation to other forms of secretion, little at present can be said. The work of Cannon, Carleson, Crile, and others, has opened our eyes to the extent to which glandular and muscular activity are called into play in the simplest forms of emotional response. The human psychologist has too long subordinated everything to the obtaining of a vocal response from the subject, while the animal psychologist has too long subordinated all to the obtaining of response in the muscles used in locomotion. Both have failed to work out methods for observing the finer changes that go on in that large class of actions that we call *emotional*. Until recent years we have been lacking proper indicators of such changes. I believe that the conditioned secretory reflex, in one form or another, can be made useful in these fields.

While recognizing the importance to all psychological students of Pawlow's work on secretion reflexes, our own work has centered around the conditioned motor reflex of Bechterew, since we find for his method an immediate and widespread usefulness.

B. The Conditioned Motor Reflex (*Bechterew*)

The *conditioned motor reflex*, while familiar in a general way to everyone, has not, so far as I know, engaged the attention of American investigators. This is not surprising in view of the fact that all of the researches have appeared in Russian and in periodicals which are not accessible at present to American students. At least we have not been able to obtain access to a single research publication. The German and French translations of Bechterew's 'Objective Psychology' give the method only in the barest outline. Bechterew's summary was the only guide we had in our work at Hopkins.

We may give a few examples from daily life of conditioned *motor* reflexes. In the moving picture tragedies the suicide of the villain is often shown. Usually the hand only of this unfortunate individual is displayed grasping a revolver which points towards the place where his head ought to be. The sight of the movement of the hammer on the revolver brings out in many spectators the same defensive bodily reaction that the noise of the explosion would call out. Again we find in persons recently operated upon numerous reactions such as deep inspirations, cries of pain, pronounced muscular movements, the stimuli to which are the cut and torn tissues themselves. For many days after the disappearance of the noxious stimuli the reactions will appear at the slightest turn of the subject's body or even at a threat of touching the wound. Similar instances of this can be seen in many chronic cases. In such cases the charitable physician characterizes the patient as having "too great a sensitivity to pain." The patient, however, is not shamming in the ordinary sense: conditioned reflexes have been set up and the subject makes the same profound reactions to ordinary attendant stimuli that he would make to the noxious stimuli themselves.[1]

For almost a year Dr. Lashley and I have been at work upon the production and control of these reflexes. We are not ready to give any detailed report of the results. Our efforts have been confined rather to the general features of the method. We find little in the literature upon such important points as:

(1) Technique of method;
(2) Subjects upon which the method may be used;
(3) Present range of application of method.

(1) *Technique of Method.*—As Bechterew's students affirm, we find that a simple way to produce the reflex is to give a sound stimulus in conjunction with a strong electro-tactual stimulus. Bechterew's students use the reflex withdrawal

[1] I wish I had time here to develop the view that the concept of the conditioned reflex can be used as an explanatory principle in the psychopathology of hysteria and of the various "tics" which appear in so-called normal individuals. It seems to me that hysterical motor manifestations may be looked upon as conditioned reflexes. This would give a *raison d'etre* which has hitherto been lacking.

of the foot: the subject sits with the bare foot resting on two metal electrodes. When the *faradic* stimulation is given the foot is jerked up from the metal electrodes. The movements of the foot are recorded graphically upon smoked paper. We modified this method slightly in our first experiments. We found that the reflex appeared more surely and quickly if the subject lay on his back with his leg raised and supported by a padded rod under the knee. This position leaves the muscles of the lower leg in a more flexible condition. As a further modification we placed one electrode having a large surface under the ball of the foot and a second electrode only one sixteenth of an inch in width under the great toe, and then strapped down the foot across the instep. When the electrical stimulation was given the great toe was raised from the narrow metal strip (toe reflex). This device made the recording of the reflex somewhat easier. While the use of the foot is fairly satisfactory it is inconvenient for general laboratory work. We found that the reflex appears in the finger as readily as in the toe. So satisfactory and convenient is this last method that we have adopted it in all of our later work with human subjects (Figs. 2 and 3). A bank of keys is provided which enables the experimenter (he is in a different room, of course, from the subject) to give at will the sound of a bell coincidently with the current, or separate from the current. In beginning work upon any new subject we first sound the bell alone to see if it will directly produce the reflex. We have never yet been able to get the reflex evoked by the bell alone prior to the electro-tactual stimulation (Plate I, a). We give next the bell and shock simultaneously for about five trials; then again offer the bell. If the reaction does not appear, we give five more stimulations with the bell and current simultaneously—etc. The conditioned reflex makes its appearance at first haltingly, *i. e.*, it will appear once and then disappear. Punishment is then again given. It may next appear twice in succession and again disappear. After a time it begins to appear regularly every time the bell is offered. In the best cases we begin to get a conditioned reflex

after fourteen to thirty combined stimulations (Plate I, b). We have found several refractory subjects: subjects in which even the primary reflex will not appear in the toe when the current is strong enough to induce perspiration. Whether this is due to atrophy of the toe reflex through the wearing of shoes, or to some other cause, we have never been able to determine. In such cases, however, we can rely upon the breathing which we record simultaneously with the reflex toe or finger movement. The breathing curve is very sensitive and a conditioned reflex appears very plainly upon its tracing.

Some General Characteristics of the Reflex

It is interesting at this point to treat of certain characteristics of the reflex. *First*, as regards the similarity and difference between the conditioned reflex and the primary reflex upon which it is grafted. However much they may differ so far as the central nervous pathway is concerned, the general and coarser motor features are closely similar. One watching the movements of a subject first beginning to show a conditioned reflex cannot tell whether he is being stimulated by the bell alone or by the bell and punishment combined. The conditioned motor reflex is usually sharp, quick, and widespread, the whole body as a rule being brought into the reaction at first. Gradually the reflex becomes more circumscribed. This appears clearly in Plates II and IX.

Second, as regards persistence of the reflex; after the reflex has once been thoroughly established it carries over from one day's experiments to the next for an indefinite period. Sometimes a single punishment at the beginning of a day's work is necessary to cause the reflex to make its appearance. We are not able to state over how long a period of time the unexercised rêflex will persist. In one case we trained one subject thoroughly in May to the bell, then did not test him again until October. The reflex did not appear on the first ringing of the bell alone, but after the first administration of the combined stimuli (at which the subject disrupted the apparatus although the induction shock was very weak) the conditioned reflex appeared regularly to the bell alone.

Third. We had hoped to make some statements concerning the reaction times of the fundamental and the conditioned reflex. While we are at work upon this problem, we are not ready to make any report as yet.

Fourth. We know that the conditioned motor reflex can be made to undergo reinforcement and inhibition by factors such as those Yerkes has made us so familiar with in his work on the mutual relations of stimuli in the reflex movements of the leg of the frog. A few examples of the rôle such factors play in the control of the reflex may be of interest. Take first the fatigue of the reflex. A well trained subject will react regularly for an indefinite period of time to a stimulus given at an interval of four to five seconds. If now we give the stimulus, *i. e.*, the bell, every two seconds for a short time, he may react for the first three times and then fail. If the interval is then lengthened, or a rest period introduced, the reflex will again appear.[1] It will be seen later that we utilize this principle of fatigue in setting up differential reactions. Oftentimes before the conditioned reflex is thoroughly set up, it will, after a time, begin to decrease in amplitude. Whether the time is increased is not known. When the reflex is beginning to vanish it can be strengthened in a variety of ways, the most usual way being the introduction of the current, but it can be reinforced also by throwing in simultaneously with the bell some other form of stimulation. I have dwelt at some length upon this subject for fear some might advance the view that the conditioned reflex is nothing more than the so-called "voluntary reaction." The fact, in addition to those cited above, which makes such a view less easily held, is the ease with which the conditioned motor reflex can be set up in animals. The strongest argument against such a point of view is the fact that it apparently can be set up on processes which are presided over by the autonomic system. To test this we have made a series of experiments having for their object the establishment of a pupillary reflex by the combined stimuli of a very strong light and a sound (bell). We found that the diameter of the pupil

[1] Plate III, a and b.

under constant illumination with fixation is very steady after the first five minutes; consequently it is possible to make measurements upon the pupil. To ordinary stimulations (sounds, contacts, etc.) there is a slight but not constant change in diameter (at times changes follow evidently upon intra-organic stimulation). But to such stimulation the pupil may respond either by dilation or constriction. In the short time which we had for training subjects we found two individuals in which, after fifteen to twenty minutes' training, the sound alone would produce a small constriction of the pupil in about seventy-five per cent. of the cases. In two subjects no such reflex could be built up in the time we had to devote to them.

The use of the pupil is thus not very satisfactory: first because it is very difficult to obtain the reflex in it; second, because, due to the fact that we have to induce the fundamental reflex by light, it is not possible to use light as a form of secondary stimulation; and third, because the method is very uncomfortable for the subject. Indeed the long training necessary to produce the reflex in refractory cases would probably be actually injurious to the eyes. Our interest in establishing a conditioned pupil reflex was entirely theoretical.

We have also made one brief attempt to establish the reflex on the heart beat; but on account of the fact that respiratory changes show so markedly on the tracing of the heart, we have been unable to convince ourselves that we have produced a genuine conditioned reflex.

Finally, we had hoped to combine this work with the so-called psycho-galvanic reflex in such a way as to produce a method which would yield quantitative results. It seemed a reasonable train of argument to suppose that the sound of an ordinary bell would not cause changes enough in the bodily resistance (or E.M.F.) to produce galvanometric deflections; but on the other hand, that the sound of the bell joined with the faradic stimulation of the foot (punishment) would produce an emotional change sufficient to show. We argued further that if punishment and bell were then given together for a sufficient number of times, the bell alone would come

finally to produce bodily changes sufficient to show on the galvanometer and we would thus have our conditioned reflex. The only fault to be found with such a train of reasoning is that it does not work out when put to practical test. In the first place the bell, as we expected, does not produce observable changes (nor do other ordinary stimuli), but, and this was unexpected, neither does the combined stimulus of bell and electric shock. Violent stimulations such as the bursting of an electric light bulb, burning the subject with a cigarette, tickling with a feather, etc., do, in our set-up (which contains no battery), produce anywhere from ten mm. to one hundred mm. deflection. Furthermore, the movement of the galvanometer does not start until an appreciable time after the stimulus has been given; sometimes not until three or four seconds afterwards (showing that effect is a glandular change). Another difficulty is that after a deflection has been obtained the original reading of the galvanometer cannot again be duplicated (resistance of the body not going back to the same point). It was largely because of these factors that we temporarily discontinued our experiments in this direction.

Method of Using Reflex to Obtain Differential Reactions

As I have sketched the method of using the conditioned reflex it is suitable for working out many problems on reinforcement, inhibition, fatigue, intensity of stimulation necessary to call out response under different conditions, etc. The method, however, has a much wider sphere of usefulness. If we take a subject in whom such a reflex is established to a bell or a light, he will react to any sound or light not differing too widely in physical characteristics. By continued training it becomes possible to narrow the range of the stimulus to which the subject will react. For example, if we train on a given monochromatic light, using red until the reflex is well established, and then suddenly exhibit green or yellow, the reflex appears. The sudden throwing in of the green light will often cause the reflex to fail the next time the red light is given. We proceed then to differentiate the reflex.

As was suggested above we bring about differentiation by punishment with the positive stimulus (red in this case) but never with the negative stimulus (green). The second step in the process of bringing about differentiation consists in exhausting the reflex to the negative stimulus (using the factor of fatigue). This can usually be done by giving the negative stimulus four or five times at intervals of about one to two seconds. After the reaction to the negative stimulus disappears we 'rest' the subject for a few seconds, and then give the positive stimulus. If this procedure is continued long enough the differential reaction is finally perfected. The differential reaction can be so highly perfected that it becomes possible to use it with great accuracy in determining difference limens on human subjects. So far we have tested it out in the fields of light, sound, and contact with very encouraging results (see Plates IV–VII and IX).

As may readily be seen, this extension of the method gives us the possibility of objectively approaching many of the problems in sensory psychology. We give no more instruction to our human subjects than we give to our animal subjects. Nor do we care what language our subject speaks or whether he speaks at all. We are thus enabled to tap certain reservoirs which have hitherto been tapped only by the introspective method. The data which we collect in this way, while they have no bearing upon a Wundtian type of psychology, serve (as far as they go) every practical and scientific need of a truly functional psychology.

(2) *Subjects upon Which the Method May be Used.*—The range of subjects upon which the motor reflex method may be used is wide. We have tried it out in all upon eleven human subjects, one dog and seven chickens.

The method works in a very satisfactory way upon the particular dog with which we worked—a beagle of very mixed breed. In the case of the dog we stimulate the sole of the foot and record the resulting leg movement (Fig. 4). Six out of the seven chicks showed the conditioned reflex in the respiratory curve (Plate II). We failed to get the reflex in one chick. These animals are comfortably saddled with leg

strapped to a punishment grill. The breathing is recorded by means of Rouse's device. Fig. 5 shows the method in use with the great horned owl.

The adult human subjects used were chosen largely but not wholly from among the graduate students of psychology and biology. Three of the subjects used had never had any psychological training. As might be expected the ease with which the method may be used is not dependent upon the previous psychological training of the subject. We give the subjects no instructions or explanations of the purport of the experiment. It is unreasonable to suppose, however, that the adult psychologically trained subjects do not get the drift of what is expected of them as the experiment proceeds. Whether the bodily set or emotion which results from this plays any rôle in the ease with which the reaction may be obtained has not been determined. On the whole I am inclined to think now that students of physics will prove to be our best subjects since they have been trained to make fine observations of small differences in physical stimuli, without at the same time trying to make crude observations of the stimulations arising from the laryngeal or other vocal organs.

Since we began to use the finger in place of the toe we have had only one subject fail to show the conditioned reflex (a graduate student of psychology). This subject also failed to give the conditioned toe reflex. We failed to obtain the great toe reflex (conditioned) upon one other subject, when we first began our work early in the year. We have had not an opportunity of retesting this individual with the finger reflex.

Whether the method can be used widely with children has not been determined. In the course of twenty minutes we obtained the reflex several times upon an eight-year-old boy. When first punished he cried and showed some reluctancy toward having the experiment continue. One of the experimenters then sat in the room with him, and, under promise of a moving picture show after the experiment, the series was completed with smiling fortitude. When once we

get the reflex established thoroughly to the bell, our troubles with children ought to be over, since we can proceed to build up second order reflexes, that is, the bell may be used in place of the electric shock (Plate VIII).

Much to our regret we have not been able during the year to find time to try the method out in pathological cases. We hope that during the coming year we may be able to try the method out thoroughly, especially upon cases to which language methods are not applicable.

(3) *Present Range of Applicability of Method.*—For some time to come the 'reflex method' will be used mainly by the animal psychologists. I shall point out here some of the advantages this method has over the 'discrimination method' now almost exclusively used in studying the sensory side of animal behavior.

As may be easily seen from our description of the technique of the reflex method the secondary stimuli are offered to the subject serially. One of the greatest difficulties in the way of using the 'discrimination method' upon animals arises from the fact that two or more stimuli must be given simultaneously. This in monochromatic light work, to take a single example, is very serious because it calls for very complicated slits, spacing prisms, methods of reversing the positions of the colors, etc. I shall not dwell upon the difficulties of the use of the discrimination method in other sense fields. They are well known. By using the 'reflex method' it becomes possible at once to discard a mass of cumbersome machinery now used both in the manipulation of the stimuli and in the control of the animal. For a complete monochromatic light equipment we shall in the future need a single monochromatic illuminator, a smoked wedge or sector, thermal couple, and galvanometer. This replaces the entire outfit recommended in the Yerkes and Watson monograph. A similar simplification can be made in the apparatus of other sense fields, especially in audition and olfaction.

A great gain is likewise effected on account of the fact that both wild and vicious animals, and animals otherwise

unsuited because of their large size, slowness of gait, etc., may be used. Another distinct gain comes from the fact that the record is made in complete and permanent form by the animal itself. The experimenter ceases to be a factor in influencing the animal's reactions. Possibly the greatest gain comes from the fact that, if our preliminary experiments may be trusted, dependable results may be reached in a fraction of the time required by the discrimination method. The differential reaction to the two bells shown in Plate IX was obtained in the dog after four experiments, lasting twenty minutes each. Had the discrimination method been used it is probable that at least three to five hundred trials would have been required. Since only ten to twenty trials per day can be given by the discrimination method the experiments would not have been completed under fifteen to twenty-five days. A further conservation of time is effected by reason of the fact that a given animal may be used in more than one experiment on a given day. Where food is given after each individual trial, as in the discrimination method, this is absolutely impossible.

At the expense of possible repetition I shall enumerate some of the uses to which the method may be immediately applied.

1. To all forms of experimentation on light, size, form, visual acuity, etc. It is apparently the only method which will enable us to study visual after-images in animals.

2. It is apparently the only existing method of testing auditory acuity, differential sensitivity to pitch, range of pitch, timbre, etc., in any reasonable length of time.

3. It affords us, by reason of the fact that the stimuli may be given serially, a method of testing the rôle of olfaction. We know nothing now concerning olfactory acuity, differential sensitivity to olfactory stimuli, classification of stimuli, the effect of such stimuli on the emotional life of the animal, etc. Nor is it very feasible to carry out such experiments by the discrimination method.

4. The method gives a reliable means of testing sensitivity to temperature and to contact and to the fineness of localiza-

tion of such stimuli—factors which likewise cannot be determined by methods now in use.

When we recall that the reflex method can be used upon man, without modification, in solving many of the above and similar sensory problems, we must admit, I believe, that it will take a very important place among psychological methods. It may be argued, however, that this method is useful only in yielding results upon very simple sensory problems. Although I cannot here enter into the wider applications of the method, I am sure that its field will be a larger and wider one than I have indicated. I feel reasonably sure that it can be used in experimentation upon memory, and in the so-called association reaction work, and in determining the integrity of the sensory life of individuals who either have no spoken language or who are unable for one reason or another to use words—I have in mind deaf and dumb individuals, aphasics, and dementia præcox patients of the "shut in" type. If indications can be trusted the method ought to yield some valuable results on the localization and method of functioning of the various neural pathways.

In conclusion I must confess to a bias in favor of this method. Time may show that I have been over-enthusiastic about it. Certainly I have attempted here to evaluate a method which possibly cannot be evaluated properly until many investigators have had opportunity to subject it to prolonged tests.

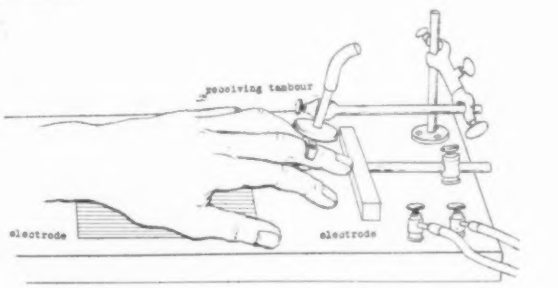


FIG. 2. Method of recording finger movement and of giving faradic stimulation. A large electrode is placed under the hand, and a small electrode under the finger. When key, in the experimenter's room, is pressed down by the operator the secondary current from the inductorium causes the finger to rise from the small electrode. A receiving tambour, to the face of which a saddle-shaped button has been shellacked, enables a graphic record to be made of such movements.

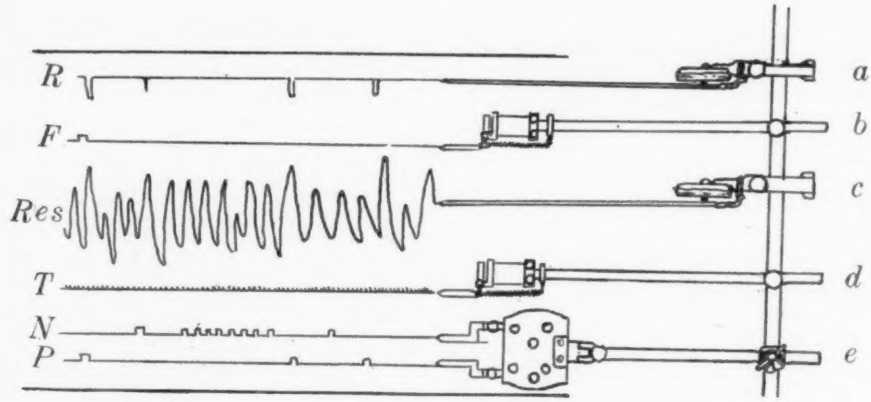


FIG. 3. Showing system of making records. *a*, Marey tambour connected with the receiving tambour from the finger (see Fig. 2). *b*, electromagnetic signal marker, connected with the key for giving faradic stimulation. *c*, Marey tambour connected with pneumograph. *d*, signal marker connected with seconds pendulum. *e*, double signal marker; one pointer moves when the negative stimulus (stimulus not to be reacted to) is given; second pointer moves only when the positive stimulus is given. The letters on the left refer as follows: *R*, conditioned reflex; *F*, faradic stimulation (punishment); *Res*, respiratory changes; *T*, time; *N*, negative stimulus; *P*, positive stimulus. These letters are used in an unchanging way in the illustrations which follow. A short schematic record of the ordinary curves obtained in the laboratory is shown. The eye should begin at the bottom and read up. The first record shows that the positive stimulus was given, that punishment was given simultaneously with it, and that the reflex occurred. The second record shows that the negative stimulus (different bell) was given, that no punishment was given with it, and that the reflex appeared (undifferentiated reflex). Then followed eight stimulations with the negative bell to produce fatigue to the negative stimulus. After fatigue to the negative bell, the positive bell is given. No punishment was given but the reflex appeared. Then the negative bell was given and no reflex appeared. Then the positive bell was given with the appearance of the reflex (differentiation). It will be noticed that respiratory changes show at every stimulation. Both bells cause a deep inspiration, increased amplitude, and a slowing in rate. When training is continued long enough, differentiation occurs in respiration just as it does in the finger movement (see Plate II, *b*); that is, in a short time, only the positive bell can produce the changes shown in this drawing.

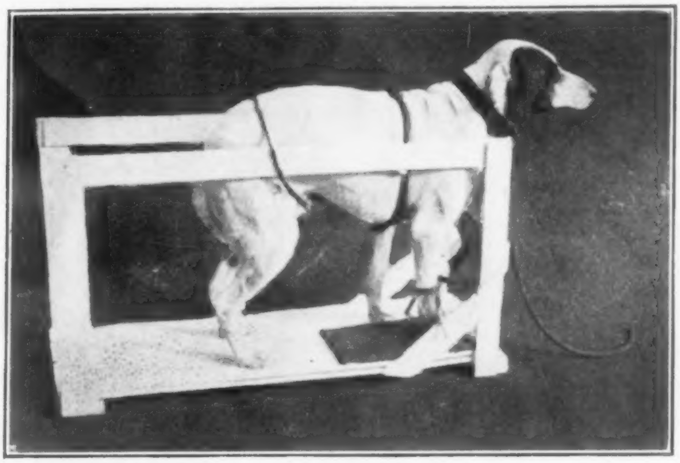


FIG. 4. Shows method of obtaining reflex with the dog. A light spring keeps the foot of the dog pressed down upon the punishment grill. This spring is so light that the dog has no difficulty in pulling up the foot when the faradic stimulus is given. A small receiving device (made like a pneumograph over a coiled spring) or a lever system may be used for the recording of the foot movement.

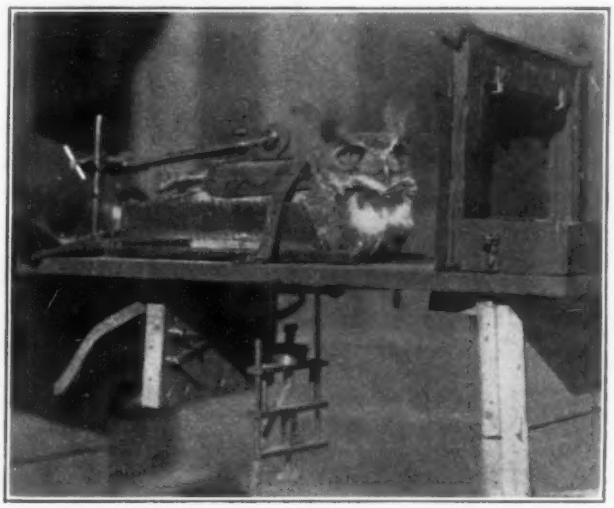
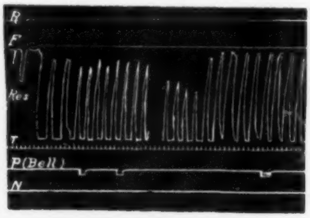
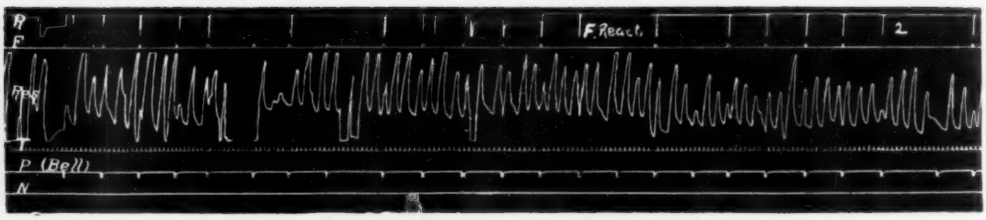


FIG. 5. Method of obtaining respiratory reflex in all birds. The great horned owl is shown resting comfortably in a padded wooden saddle. Underneath the floor of this apparatus Rouse's respiratory apparatus is shown, sliding on vertical rods. A V-shaped button is shellacked to the receiving tambour, which is adjusted lightly against the bird's chest. The owl's feet are attached to a punishment grill.

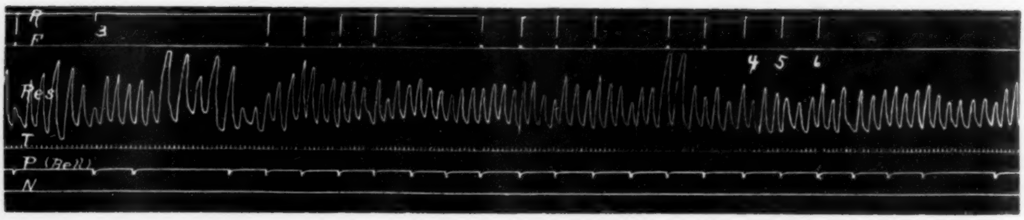
PLATE I. Formation of conditioned motor reflex to sound of bell.



a. No reaction to bell alone.

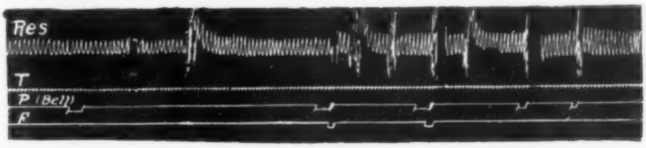


b. Reaction to bell alone (F. React.) after 13 combined stimulations (bell and punishment).

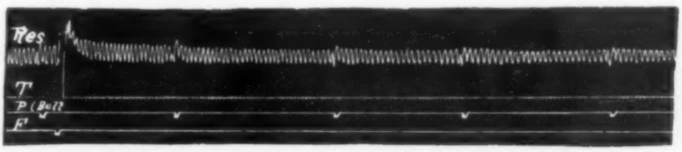


c. Reflex more firmly established as shown by three reflexes, 4, 5, 6, appearing in succession without punishment. (Further training is necessary.)

PLATE II. Conditioned respiratory reflex to sound (bell) in the fowl.

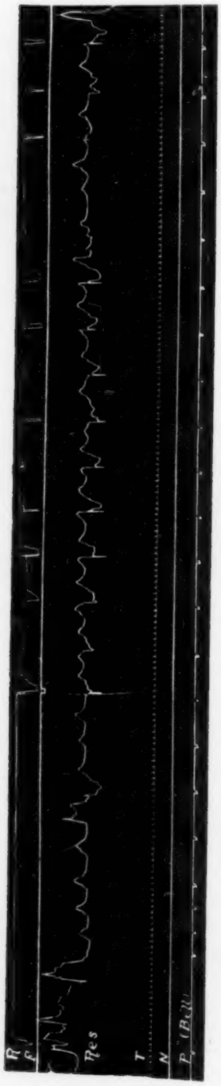


a. Conditioned reflex at beginning of training: Respiration obscured by general motor activity.

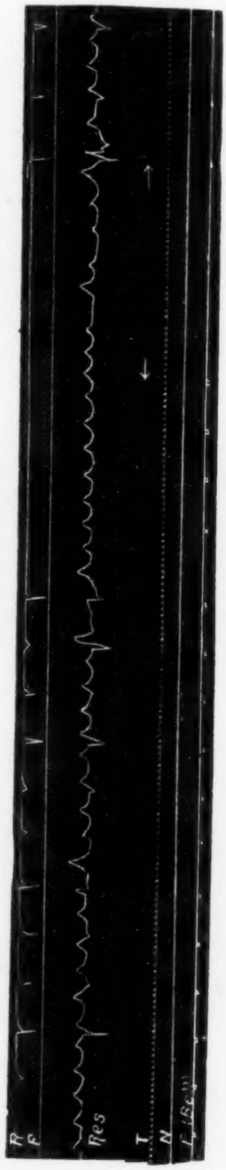


b. Restriction of reflex to respiratory movements after long training.

PLATE III. Reinforcement of conditioned motor reflex. (These records are from trained subjects. Occasionally for one reason or another the reflex will disappear.)

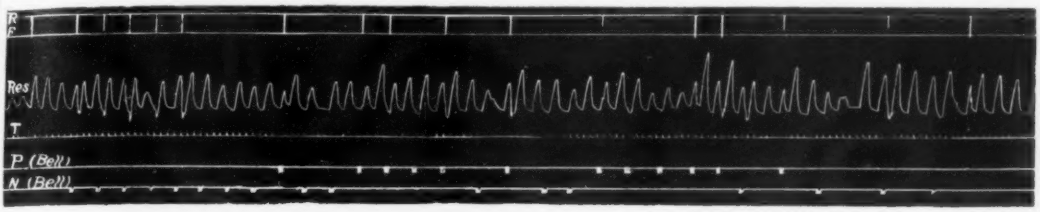


a. Reinforcement by single punishment.

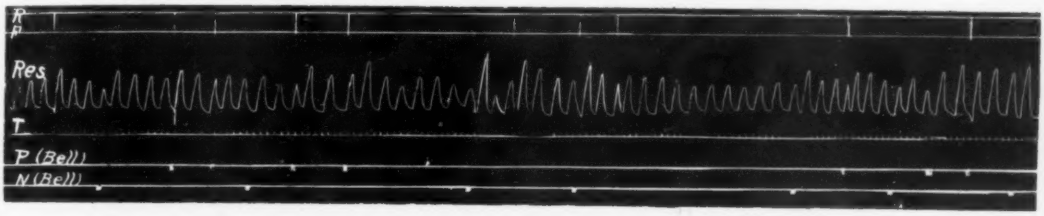


b. Reinforcement by period of rest (between arrows).

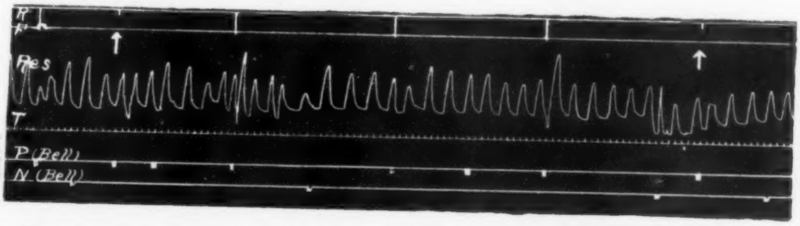
PLATE IV. Last stages in the formation of a differential reaction to sound of bells of different pitch.



a. Fatigue of reaction to negative bell.

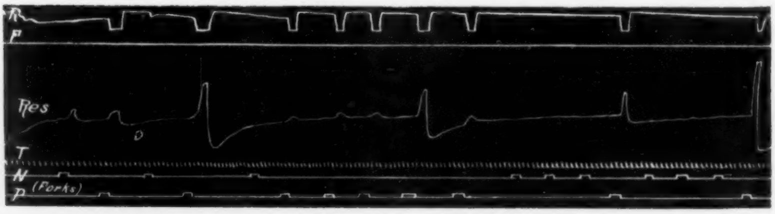


b. Differential not firmly established.

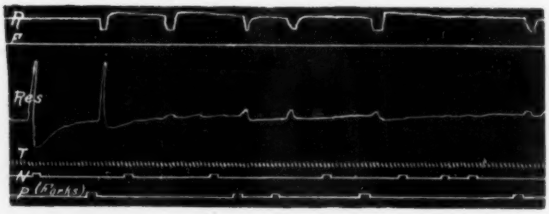


c. Reaction almost perfected after final punishment with positive bell. The arrows indicate that the reflex is present though small.

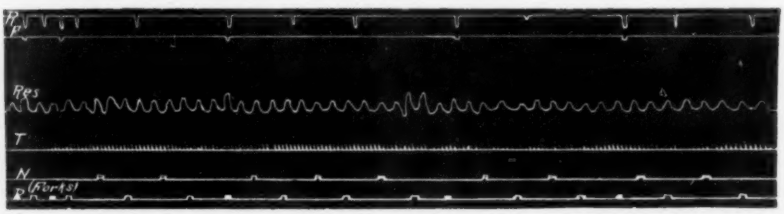
Plate V. Differential reflex to pure tones. (Standard fork 256 v.d.)



a. Perfect differentiation when the difference is 6 v.d.

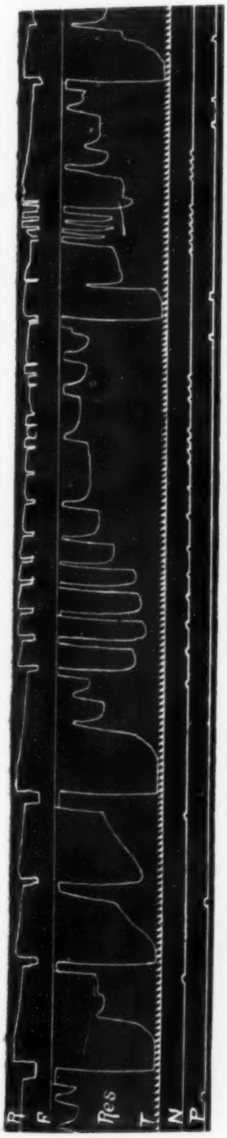


b. Perfect differentiation when the difference is 3 v.d.

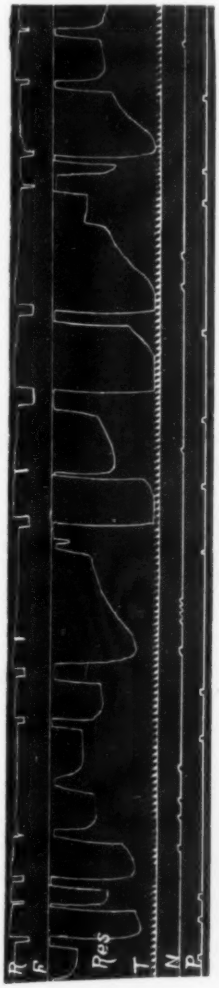


c. Differentiation, in another subject, when the difference is 6 v.d. (This subject failed when the difference was 3 v.d.)

PLATE VI. Differential reflex to two contact stimulations on human forearm.



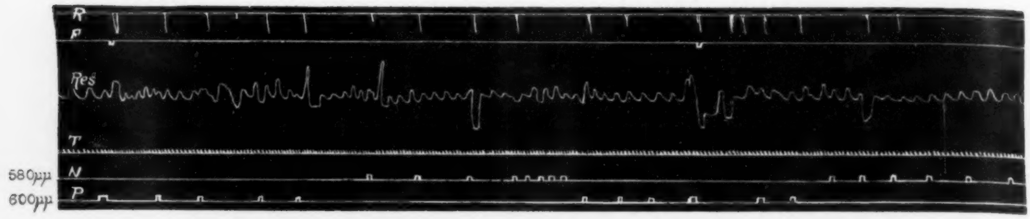
a. Reflex established but not differentiated: differentiation brought about by fatigue of reflex to negative stimulus.



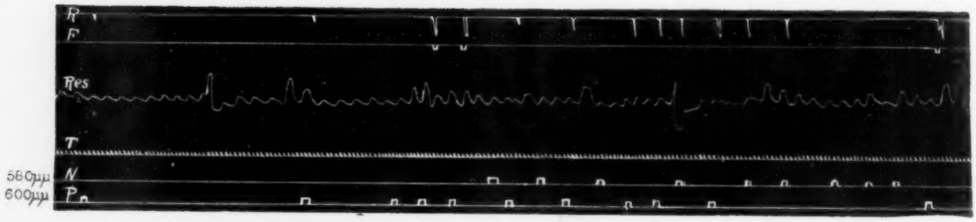
b. Beginning of differentiation.

c. Differentiation established (arrow shows that reflex was present in tracing but too faint for reproduction).

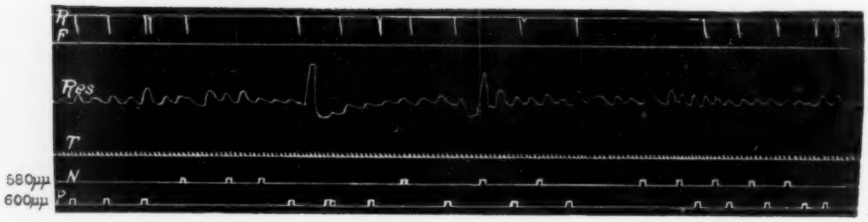
PLATE VII. Differential reflex to lights of different wave-length.



a. Reflex established but not differentiated.

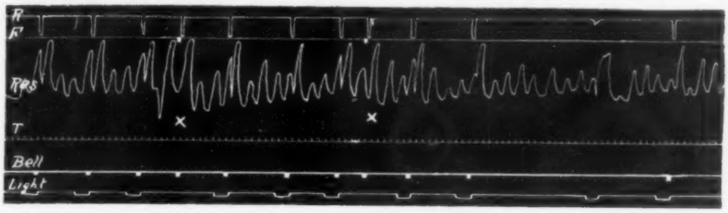


b. Progress toward differentiation. Example of reinforcement by rest.

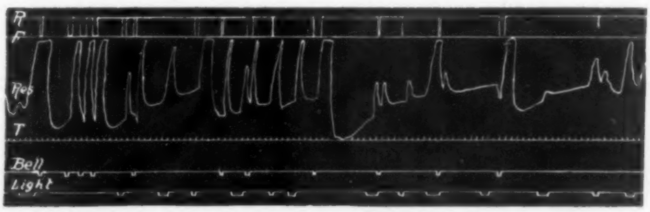


c. Differentiation perfect.

PLATE VIII. Formation of conditioned reflex to light by association with conditioned reflex to sound. (The reflex to sound had been set up previously by the use of punishment.)

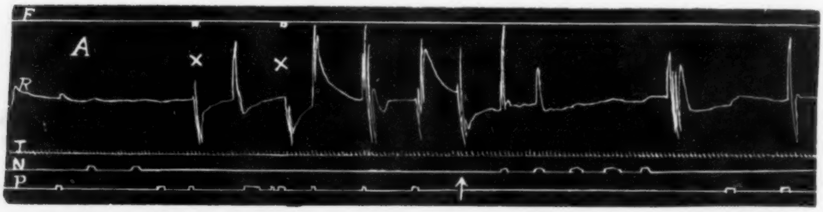


a. Light stimulus combined with sound. Reflex follows with light alone. (*Sound* stimulus *reinforced* by punishment at *x*.)

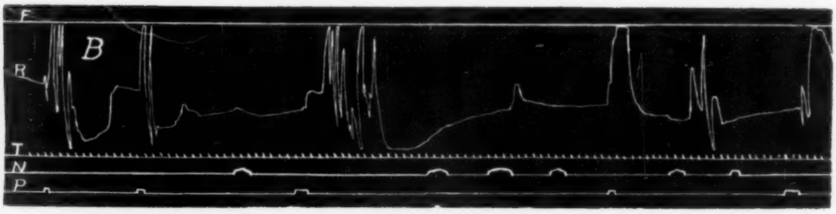


b. Later stages of training. Respiratory reflex well established.

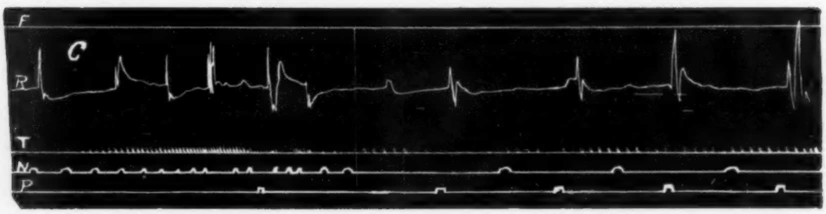
PLATE IX. Rise of differential reaction in the dog to two electric bells of different pitch. (*R*, reflex; upward jerk of forefoot.)



a. Undifferentiated reaction after punishment. (Punishment shown at *x*.) Arrow shows rhythmical reaction, no stimulus having been given.)



b. Beginning of differentiation.



c. Differentiation established. (Prolonged stimulation with negative bell until fatigue. This seemed to complete the process of differentiation.)

THE BIOLOGICAL POINT OF VIEW IN PSYCHOLOGY AND PSYCHIATRY

BY E. STANLEY ABBOT

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The advantage and necessity of taking the biological point of view in psychology and psychiatry have been urged in recent years by psychologists and especially by Dr. Adolf Meyer and other psychiatrists. To this necessity the writer heartily agrees. For this point of view makes the study of these subjects objective and as free as possible from metaphysical bias and *a prioristic* theories. It also sets more definite and workable problems, and establishes a criterion for evaluating both old and new theories and conceptions.

Because this point of view seems to the writer to have been imperfectly conceived and inadequately applied, and because it and its implications have been nowhere fully set forth, this attempt to supply the deficiency is made.

If we regard biology only as the science of matter in the living state, it is limited to studies of structure and its physiological activities; but if we take the broader, and in the writer's belief truer meaning, biology is the science of living things. This includes not only studies of structure but of all the activities of the biological unit, including its conduct or behavior. Jennings's studies of the behavior of the lower organisms belong to biology just as much as do Loeb's physiological studies.

The fundamental differences between non-living and living things are that the latter by internal activities *make themselves* out of the materials of their environment, and *reproduce their kind*. In a very limited sense even the non-living thing may be said to *adapt itself to the environment*, in that it reacts by its own inertia to the forces that act upon it, but not by any internal self-directive activities of its own. In the world of living things there is the same reaction of inertia

as in non-living things, but in addition there are internal activities, some of which are of a self-directive nature.

In the vegetable world these internal activities are little, if any, modified by the plant itself, though some botanists hold that root-caps and vine tendrils, for example, exercise a direction of the plant activities which is not wholly explained by the various tropisms to which the plant is subject.

But in the animal series, as we ascend the evolutionary scale, the self-directive internal activities assume a progressively greater prominence in the reactions of the individual to its environment. The life of the individual biological unit consists in the continuous adaptation of itself to its environment as well as it can. That is its job in life. If it absolutely stops reacting by internal activities, it dies. If it does not react as well as it can, it succumbs to external agencies or does less well than its neighbor. (Even we human beings try to justify ourselves and others when we have done less well than we might, and we rarely intentionally do less well than we know at the moment of acting, though often afterwards we see how we might have done better.) This self-adaptation is minimal in the resting stage of spores and seeds, and in the higher animals in hibernation and in sleep, and is maximal in the wide-awake activities of the higher animals, especially man.

We may say then that from the biological point of view man is a biological unit reacting as well as he can to his environment by means of internal self-directive activities which determine his outward acts. Many of these internal activities are physiological, but most of these which result in his external behavior or conduct are psychological. All of his activities are directed to the great end of his best self-adjustment to his whole environment, though lesser or nearer and more concrete ends are usually more immediately prominent to the individual. That is, he usually reacts to the immediate present situation without much thought of his reaction being an integral and essential part of a large adjustment to life, *i. e.*, to his whole environment, including his whole past, present and future situations, experiences and reactions.

The psychical activities of any biological unit, then, are links in the chain of internal reactions by which the unit adjusts itself or reacts to the forces which act on it and the situations in which it finds itself. Each link is a reaction, effect of preceding links, cause of succeeding ones. In psychology then we not only have to study the special psychic act or type of acts as a separate entity or process, but we should always study also its immediate and remote causes and effects. Study of the causes leads back by one route to factors of the environment and by another to anatomical structure and physiological process. Study of the effects leads forward in one direction to behavior, *i. e.*, action in and upon the environment, and in another to bodily changes and processes.

From the biological point of view then every psychic event is a reaction,—a link in the chain, or a moment in the series, of reactions by which the individual adjusts or adapts himself to the environment. The nervous system (or, in the unicellular organisms—if any of their reactions deserve the name psychic, as the writer thinks they do—the body protoplasm and outer membrane) is the specially adapted structure for the performance of the psychic functions or processes. Mind is the abstract name we give (1) to the capacity to react in certain, *i. e.*, psychic, ways, (2) to the organized whole of any individual's psychic reactions, or (3) to the content of any individual's psychic reactions, especially ideational ones. It is in reality a function or set of functions—activity or types of activity—but through misconception it is often used to indicate some mysterious thing which can act of itself or is opposed to or contrasted with body, and it is often referred to as having structure. (See for example, McDougall, 'Mind and Body,' pp. 165, 330.) From a strictly biological point of view it bears the same relation to brain and to the individual that respiration does to lungs and to the individual, or that running does to legs and to the individual. It is the *individual*, not the brain, that thinks or exercises the other psychic activities we call mind, just as it is the individual, not the lungs, that breathes, or the

individual that runs, not the legs. But by means of the brain, the lungs and the legs, the individual thinks, breathes, runs.

We do not think of opposing or contrasting respiration or running with lungs, legs or body. Neither should we do so with brain or body in the case of mind. Similarly it is absurd for us to think of respiration as identical with lungs, or running as identical with legs. It is equally absurd to say, as the identificationists still do, that mind is brain, and brain is mind. To speak of a mind-stuff (except in the sense that we may speak of electrical-stuff or light-stuff on the theory that there is no substance but energy, and the writer has never seen it used in this sense) is equally misleading, and savors of the anachronistic notion that mind is secreted by the brain as bile is by the liver—a notion whose absurdity becomes apparent if we again use the comparison of the lungs secreting respiration, or the legs secreting running.

But this view does not mean that we should neglect the brain or nervous system or body, any more than we should in studying respiration or any other function. It is true that we can study many aspects of psychical as well as other functions with only a slight or superficial knowledge of the organs that subserve them. But in physiological psychology some structural knowledge is essential; in comparative psychology the degree of evolutionary development of the nervous system is seen to affect the capacity for psychic reaction; and child psychology is different from adult psychology on account of the individual development, and it is advantageous to have knowledge of these structural conditions. So intimately are structure and function related that it will doubtless be found eventually that racial, family and even individual traits are partly dependent on more or less minute structural differences in brain architecture and nerve-cell distribution.

In that branch of abnormal psychology which is called psychiatry, in which the scientist has not only to understand the psychological reactions but has to treat the patient also, a knowledge of the brain, nervous system and body in general

is especially needful. The effect on mental processes of such bodily conditions as fatigue, toxæmias (whether from exogenous or endogenous sources), and brain lesions needs only be mentioned, to have its importance recognized. On the other hand, the effects of certain psychical processes, notably some of the emotions, on bodily activities have recently been emphasized by Crile, Cannon and others. In the writer's belief the affects intermediate between the ideational activities on the one hand and the bodily activities on the other, and this action, while usually and most forcibly exerted in the direction indicated, is nevertheless exerted to some extent in the reverse direction—that is, bodily condition acts to some extent on the ideational processes and content through the affects. This is seen especially in the insane. When we see how some of the reactions belonging to two great functions, as digestion and circulation, may mutually affect each other, it should not be a matter for surprise or wonder that some of the reactions of another great function, mind, should affect and be affected by other bodily reactions or functions. From the biological point of view the relations between body and mind are in principle almost as simple as those between body and any other function. The developmental stage of the structure as to both its phylogeny and its ontogeny and the degree of its integrity will determine the capacity of the individual to react in its racial way.

It was said above that in tracing backward the causes of any psychic event we come eventually to the environment. This as a cause of psychic activity has been too much neglected.

It is the work in life of the biological unit to adjust itself to the environment. This means that the environment, or its various factors, must act upon the unit, and that the latter *re*acts to it. Any given unit will react to the extent of its capacity for reacting, and this is determined by its structure. The greater the number of environmental factors to which it can react, the more complete and adequate will be its response. It is the nature of the environmental factors that has determined the types of response of which the unit is capable.

What are the great types of factors and how are they arranged in the environment?

I need not dwell on *matter*, which we call substance, and the *modes of energy* which we call light, sound, heat, etc., as such types of factors which are met with in the concrete materials and forces of the world we live in.

But as factors there are also other *living creatures*, with their *behavior*, and the *content* of their psychic processes, their *thoughts* and *feelings*, especially in man and the higher animals. Predatory animals and their prey, for example, both have to react to the respective desires and behavior of the other. The content of thinking and emotion in man, whether stored in literature or expressed in speech and action in the presence of the responding human, are large factors to which he must respond. They are outside of him, actually existent in the environment. His own past experiences, including his hopes, ambitions and decisions for his future, are actual former occurrences to which he may react in the present, not only by recollecting them but in many other ways, including the predetermination of many future reactions.

Relations are also objective existences to which we human beings at least react. These are not only spatial and temporal, but of inherence, as of the thing in its kind; genetic, as of offspring to parent; social, as of husband to wife, or individual to community; business, as of debtor and creditor; and innumerable other kinds. There has been evolved, in man at least if not in other animals, a capacity to be more or less sensitive to such environmental factors. We need further study of the mechanisms of our sensitivity and reactivity to them.

Similarly, there is *law* or *necessity* as a factor or group of factors. The sword of Damocles *must* fall if the thread breaks; the walls and roof of the subway must fall in if they are insufficiently shored up; etc. We search for innumerable physical, chemical, genetic and other laws or necessities, and we see them constantly operative in the world about us. It is because they actually exist objectively that there has been evolved in us the capacity for responding to them, not

only as being subject to them, *i. e.*, compelled to obey them, but as being sensitive to them, comprehending them. Psychology will benefit by a study of them as stimuli as well as of the mechanisms by which we respond to them.

Further, there are *obligations* and their reverse *rights*, which are objective existences, at least for us human beings. Whenever two men stand in the relationship of debtor and creditor, there is the obligation of the one to pay, the right of the other to be paid; in the parent-offspring relationship there is the obligation of the parent to protect the offspring till the latter is able to lead its own independent existence, and the right of the offspring to such protection; in the relationship of man and other animals to the respective life activities of others some of us recognize the right of all to live their lives as they please provided they don't interfere with others' rights, some of us recognize such rights only in man, some of us only in white men, some of us only in the dominant white man, or superman, etc., and each of us sees his own obligations according to the limits of his vision. It is the part of ethics to study what those and many other rights and obligations are, of psychology to study the mechanism by which we are sensitive and responsive to them. But psychology cannot adequately study the mechanism without a knowledge of the nature of the stimulus any more than physiology can adequately study the mechanism of digestion without a knowledge of the composition of food-stuffs.

Such, to the writer's mind, are the great types of environmental factors. How are they arranged in the environment?

Every biological unit is not only in an environment, consisting of these factors, but each one is at the center of his own environment, and is himself a part of it. It may be regarded as consisting of a set of concentric circles or spheres, each representing a limited situation, the factors of which act with greater or less force upon the unit at the center, and to which the unit responds with more or less activity, physical and psychical. The limits of the circles are very indefinite, one shading into the other. To illustrate, let us consider a patient in a hospital for the insane. Any con-

ceivable situation in life would serve, but abnormal conditions sometimes help to illuminate normal ones. What is his environment?

He is himself at the center of it, his own body, thoughts and feelings being part of it. Next exterior are the room, its walls, windows, doors, the furniture, the warmth, the daylight or lamplight, the people moving about, etc. If the patient is clear, these may be correctly responded to; if he is confused or hallucinated, they may not be. In the next larger circle people speak to him or to each other. He sees books and newspapers about. He may understand what is said or printed, or he may not. Then there are the uniformed women (nurses) and doctors and patients, each having a different relation to each other, the place and to him. He may comprehend these relationships, or may not, in which case he will be puzzled and perplexed. He may recognize the nurse as a nurse, the doctor as a doctor, and be able to call them by name, yet not recognize that he is a patient, that he is in a hospital, etc. He recognizes the terms involved in the relationship, but not the relationship itself. He can see and recognize 2 and 3 but cannot put them together to make 5.

A wider circle of his situation is that he is sick and in need of care—some of his mental activities are modified, his behavior has been different, he cannot control his thinking or his feeling or his conduct as usual. He may have recognized this and come of his own accord to the hospital, or he may not have recognized it, and others had to bring him against his wish and opposition.

Next, he is a business man and a father of a family. He may realize his obligations to his business associates, customers, creditors, and to his wife and children, or not. They and a host of related conditions form parts of his situation.

He may have done, or think he has done things in the past—such as acquire some venereal disease—which modifies his present condition or thoughts. One can go on to show indefinitely wider spheres of environment.

This illustration is not intended to show the limits of the

widening circles of environment, for there are no clear-cut ones, but only to indicate that the environment may be regarded in that way. In comparative psychology and in psychiatry we need to know how large is the environment and to what kinds of factors in it the individual we are studying is capable of reacting.

The factors of the inner circles of the environment, the immediate surroundings, mostly concrete, are constantly changing and shifting, requiring constant adjustment on the part of the individual. The remoter ones as a rule change less, somewhat in proportion to their remoteness or abstractness, though not absolutely so. They all have their influence and effect on the conduct of the individual, through the psychological processes involved in his perception and comprehension of the various factors of the whole situation and of their relative importance at the moment of acting. Other psychological activities of course enter into the final behavior or reaction to the environment, such as various affects, plans, decisions, will-impulses, etc.

In psychiatry especially it is necessary to take into account, not only the patient's psychical reactions, but all the bodily conditions that may modify them, and particularly all the environmental factors both immediate and remote, that make up the successive situations in which the patient is and has been. It is in this field that Freud and his followers have taught us so much. It is partly due to that influence that we are realizing more and more that the aim of the psychiatrist should be to study the patient's *total reaction to his total environment.* The biological point of view makes it insistent however that the study of this total reaction include that of the somatic factors which may modify it, a point that is apt to be somewhat neglected on account of the interest of the psychical reactions. Psychiatry becomes not only a medical science, but an intensive individual psychology as well.

The biological point of view—that every psychic event is a reaction of an individual—if consistently followed and applied, will correct a tendency, prevalent to some extent in

most if not in all psychologies, very common in James's psychology, and fairly running riot in the writings of the Freudian school, to personify, as it were, or to make independently acting entities of, the psychical functions. For example, Yerkes,[1] describing an effect of habit, writes that while talking with his friend, ". . . the series of acts gets itself performed . . ." Münsterberg[2] says: "Each man lives in the world which his inner dispositions select and shape." Baldwin,[3] writing of association, says: ". . . a group of processes, . . . conspire, so to speak, to 'ring up' one another," and he subsequently speaks of the 'conspiracy' and the 'conspiring elements.' James says: "The psychologist gets to supposing that the thought of an object knows it in the same way that he knows it,"[4] and 'the thought that thinks it,' and 'thoughts . . . know objects,'[5] etc., while Freud notoriously introduces a 'censor' and speaks of libido, of dreams, dream-wishes, and various thoughts, affects and abstractions as though they were endowed with independent initiative and activity. Making all due allowance for a proper use of analogies and of abstractions to avoid descriptive phrases and periphrases, and for literary leavening of an otherwise perhaps heavy dough, there yet remains enough of such usage to indicate a haziness of conception on the part of the writers, and to becloud for the reader a subject not too clear at best—not to mention its scientific inexactitude.

For the same biological point of view the scientifically sterile conception of a 'stream of consciousness,' with the pseudo-problems that it raises, does not exist (except as a fancy), any more than an analogous conception of a 'stream of respiration' could exist. In sleep the organism reacts psychically to a slight degree, in unconsciousness it ceases to react at all in those ways, though it continues to react in most physiological ways. The unity of the 'ego' is deter-

[1] 'Introduction to Psychology,' p. 383.

[2] 'Psychology, General and Applied,' p. 221.

[3] 'Mental Development,' p. 266.

[4] 'Principles of Psychology,' Vol. I., p. 196.

[5] *Ibid.*, p. 197.

mined by the facts that it is the same organism which reacts at successive times, that each experience is recorded in the same individual (not in any other), and that the organism can recall the content of most of these experiences by subsequent psychical activities. The partial or split personalities —wrongly called multiple personalities—are partly comprehensible on the grounds that the individual, through mechanisms of which we know little, can not recall and make use of large groups or sets of experiences, and can react in more than one way at a time.

This point of view may seem to be a purely mechanical one, and hence fatalistic, leaving no room for choice or free-will. In the sense that every act, physical, physiological or psychical, has its determinants and its effects, that no reaction is haphazard or occurs by chance, this is true. But that it leaves no room for choice is not true. Every individual, from lowest to highest, is always and inevitably in a situation, some of the factors of which are constantly changing more or less. The individual *must* react to the environment, even if only by inhibiting all external reactions. But there are many possibilities of reaction (more such the higher the individual is in the evolutionary scale), and the individual can, even must, choose which of the possibilities to carry out. The choice itself, the act of choosing, is a part of the reaction to the situation. The choice is but one of the determinants of the act chosen. There is then a compromise between free-will and determinism. The individual *must* react, but has a measure of choice—freedom of will—as to *how* it shall react, *i. e.*, as to *what* reaction it shall make.

Thus it may be said that from the biological point of view man is a biological unit reacting to his environment; that his reactions are partly psychical and partly physical or physiological; that every psychical event is a reaction; that the organized whole of the psychic reactions, or the capacity to react in psychical ways, or the content of the psychic reactions is mind, which is related to body as function or activity is related to structure; that to understand fully the psychic reactions it is desirable and in some instances neces-

sary to know the structure which subserves the function, the modifications of this structure, and the causes which modify it (and hence the function which it subserves); that it is also necessary to know the nature of the various factors of the environment and their grouping in a situation when considering the psychology of any individual's behavior or reaction to the situation; that in psychiatry we must seek to learn the patient's total reaction to his total environment.

SOME LOGICAL ASPECTS OF THE BINET SCALE[1]

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Part I.

Purpose

The purpose of this article is,

1. To show how to determine the per cent. of children who should pass a test of intelligence at the age for which it is to be considered standard.

2. To show how the tests could be standardized more accurately than merely by the consideration of the per cent. of children passing it at the age for which it is to be considered standard. This may be done by a consideration of the per cents. of children which pass it at several different ages.

3. To show three criteria of a test as a test of intelligence.

4. To suggest a method of constructing an absolute scale of intelligence and to consider the proper spacing of tests on the scale.

5. To suggest an application of the term "intelligence quotient" such that it shall be constant for any individual throughout the growth of intelligence.

The Determination of the Per Cent. of Children Who Should Pass a Test at the Age for Which It Is to be Considered Standard

In regard to the per cent. of correct responses necessary for locating a test in the Binet Scale, Dr. Terman says:[2] "This is a mooted question. Binet's standard, also that of Terman and Childs, was a shifting one, varying from sixty to ninety per cent. according to the upward character of the

[1] I wish to express my thanks to Dr. Lewis M. Terman for suggesting this study, for supplying needed data, and for making many helpful suggestions and criticisms.

[2] Dr. Lewis M. Terman, 'Suggestions for Revising, Extending, and Supplementing the Binet Intelligence Tests,' *J. of Psycho-Asthenics*, 1913, **18**, 24.

curve for the test in question. Goddard, Kuhlmann and Bobertag, on the other hand, adhere strictly to the seventy-five per cent. standard. Bobertag considers at length the justification for this rule, and comes to the conclusion that no definite proof of its correctness is available. The fact, however, that this standard gives us a distribution of mental ages for children of each group age closely approximating the so-called 'normal curve of distribution,' is, in the opinion of Bobertag, a weighty argument in its favor." Kuhlmann, however, on the basis of data which he later gathered from tests of 1,500 normal children, rejects the seventy-five per cent. theory and like Terman and Childs adopts a sliding standard ranging from about seventy per cent. at the lower end of the scale to about fifty-five or sixty per cent. at the upper end.

This lack of uniformity as to the proper per cent. for standardization appears to be due to confusion concerning the logic involved in the derivation of this per cent. from a definition of standard intelligence. The following passages will indicate (1) how standard ten-year intelligence, for example, is to be defined and (2) that from this definition the per cent. for standardization is to be derived. Terman says:[1] "Accordingly our search should be for a standard . . . which would reveal the true median intelligence quotient for non-selected children at each age. What we really want to know about a given child is how he tests with reference to the median child of his years, rather than whether or not his intelligence is exceeded by that of ninety per cent., seventy-one per cent. or sixty per cent. of the children of his age-group."

It might be well at this point to get a clear idea of what is called a 'normal' or 'symmetrical' distribution. Suppose we had the exact measures of the heights of a large group of unselected ten-year children. If a dot were placed at a height above a horizontal line corresponding to the height of each child, these dots would appear as shown at *A* in Fig. 1. If we drew a series of vertical lines representing the appearance of the children arranged in a row in the order of their heights, these would appear as shown at *B*. We would find that the dots were thickest near the middle and consequently an imaginary curve touching the tops of the heads of the children would be most nearly horizontal near the middle. We would find that as we recede from the middle in either direction the differences in the adjacent values of the heights become

[1] *Loc. cit.*, p. 26.

continually larger and that consequently the slope of the imaginary curve touching the tops of the heads of the children would become continually steeper. This form of distribution is called a normal or symmetrical distribution.

If we knew the exact values of the intelligences of a large group of ten-year children, it is quite likely that we would find them distributed in much the same manner, in which case our figure would represent this distribution also, the vertical lines representing the degrees or amounts of intelligence possessed by each. The distribution of the intelligences of nine-year children would, in all probability, be of the same form as that of ten-year children though of a slightly narrower range vertically, and that of eleven-year children the same but of slightly greater range, etc. In order to have a single representative value of ten-year intelligence we have defined it as the median value of the intelligences of all ten-year children. The median value of the intelligences of any 1,000 unselected ten-year children would doubtless be very close to the theoretical standard, and may be used as that standard for working purposes.

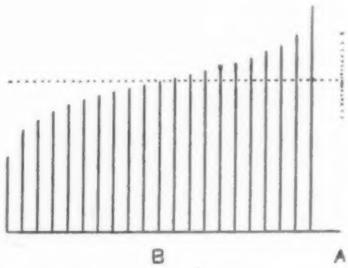

FIG. 1. Showing a hypothetical curve of distribution of intelligences for a single chronological age (ogive form).

Stern says:[1] "The controlling principle for the adjustment or standardization of age-levels is that approximately symmetrical distribution of mental ages must prevail for each level. That is, the tests are properly arranged and skillfully assembled into a system if, when a large number of normal, unselected children of a given age are tested, a large middle group stand 'at-age' and the rest are divided fairly equally between advanced and retarded cases." From these passages it will be seen that standard ten-year intelligence is to be defined as the intelligence of the median ten-year child. It would seem that the logic involved in the determination of the per cent. of ten-year-olds which should pass a test in order that it should be considered a standard ten-year test

[1] Wm. Stern, 'The Psychological Methods of Testing Intelligence,' Warwick and York, 1914, p. 100.

is embodied in the following three propositions, which will hold equally well for all ages:

1. *Standard ten-year intelligence is defined as the intelligence of the median ten-year child.*

2. *Therefore standard ten-year intelligence is exceeded by 50 per cent. of ten-year-olds.*

3. *Therefore a ten-year test will be passed by 50 per cent. of ten-year-olds.*

Possible Objections to the Above Reasoning.—There can be no question that the second proposition follows from the first. Concerning the third, however, it may be urged that a child quite often passes a ten-year test though his intelligence is below ten-year intelligence; it may not follow that 50 per cent. of ten-year-olds will pass a ten-year test even though standard ten-year intelligence is exceeded by just 50 per cent. of ten-year-olds. To this objection it may be said that if a test is passed by 50 per cent. of ten-year-olds, then by the laws of chance, it will happen as often that a child whose intelligence is above standard ten-year intelligence has failed the test as that a child whose intelligence is below standard ten-year intelligence has passed it, or, stated more accurately, by the laws of chance it will happen as often that a child will have a general level of intelligence above standard ten-year intelligence and a degree of the function employed in the test which is below the level of standard ten-year intelligence as it will happen that a child will have a general level of intelligence below standard ten-year intelligence and a degree of the function below the level of standard ten-year intelligence. Since 50 per cent. of ten-year-olds have levels of intelligence above standard ten-year intelligence, it follows that 50 per cent. of ten-year-olds will also have degrees of the function employed in the test which is above the level of standard ten-year intelligence. Therefore 50 per cent. of ten-year-olds will pass a test requiring a degree of a function equal to the general level of standard ten-year intelligence. Certainly this is a ten-year test.

A second possible objection to the foregoing reasoning is indicated by the passage quoted from Terman which reads:

"The fact, however, that this (75 per cent.) standard gives us a distribution of mental ages for each age group closely approximating the so-called 'normal curve of distribution' is in the opinion of Bobertag a weighty argument in its favor." That is, it may be urged that the results actually show that if the 50 per cent. basis be used, 50 per cent. of ten-year-olds will not be found able to obtain a mental age of ten years or better, tending to show that the 50 per cent. basis is incorrect and that a larger per cent. must be used. At first thought this would seem to be a contradiction between the facts and the logic. Let us suppose, however, that the tests were standardized on the 50 per cent. basis but that when children are tested their score is made too low by some error in the manner of scoring; then, of course, less than 50 per cent. of ten-year-olds would attain a score of ten years in mental age, that is, the distribution of mental ages would not be normal. Similarly, if the tests were standardized on a 75 per cent. basis so that ordinarily 75 per cent. of ten-year-olds would be expected to receive a score of ten years in mental age, this same supposed fault in the method of scoring might reduce the number receiving the score of ten years to 50 per cent. and give us a normal distribution of mental ages for ten-year-olds. Therefore "the fact . . . that this (75 per cent.) standard gives us a distribution of mental ages for each age group closely approximating the so-called 'normal curve of distribution'" is not at all conclusive proof of the validity of the 75 per cent. standard. In fact, the existence and nature of an actual error in the present method of scoring similar to the above hypothetical one will be shown.

A third possible objection to the foregoing reasoning is that a ten-year test is not to be considered as one which in general requires ten-year intelligence to pass it, but is merely such a test that when standardized by the proper per cent. and the series scored by the particular method now in use, the number of ten-year-olds attaining a score in mental age above ten years will equal the number attaining a score below ten years and that the distribution of mental ages of ten-year-olds would be normal with the median mental age

at just ten years. (It is quite possible for the distribution of mental ages to be normal yet with the median mental age above or below ten years.) To this objection it may be stated that, as will be shown, even with ten-year intelligence defined as above, the 50 per cent. method will give the desired results and will be found simpler than the trial-and-error method of obtaining the 'sliding standard' of per cents.

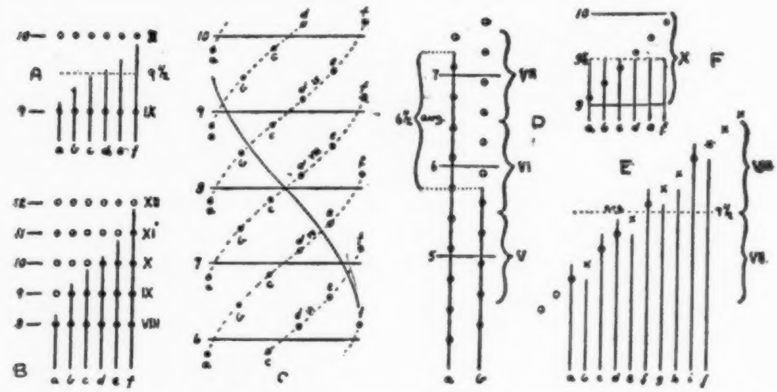

FIG. 2. Showing that by the present method of scoring the mental age of a child may be rated one-half year too low.

The Error of Scoring.—Let us assume (see Fig. 2*A*) that the six tests in group IX. measure six different functions which are represented by the letters *a*, *b*, *c*, *d*, *e*, and *f*, and that the six tests in group X. also measure these six functions. Let us assume that a child passes all the tests in group IX. but fails all in group X. We do not know how far the ability required in any test in group IX. was exceeded by the degree of the function actually possessed by the child (as represented by the vertical lines). The best we can do is to assume that some tests were barely passed while others were considerably more than passed. We should, in fact, assume that the degrees of the six functions tested were distributed according to the law of distribution, as shown in the figure. If so, the general level of intelligence as measured by the average height of the six lines would be $9\frac{1}{2}$-year intelligence, the mental age which of course the child should be accorded.

By the method of scoring now in use, however, the child would be given a mental age of but 9 years.

That this fact is overlooked by Stern, and presumably by many who use a method of scoring involving this error, is suggested by the passage (page 36) which reads: "Considering the problem schematically we might think that the grade of intelligence could be expressed by the stage whose tests could just be passed by the child; a subject who readily passed all the tests up through the 9-year ones, but failed with the 10-year and subsequent ones, would, accordingly, possess a 9-year grade of intelligence."

If we assume the distribution of specific abilities to be wider we may represent it as at *B* in Fig. 2. Here the average height of functions *a* and *f*, of functions *b* and *e*, and of functions *c* and *d* stands at the level of the ten-year tests. Therefore, the child should be given a score of ten years in intelligence, whereas by the method of scoring now in use he is given but $9\frac{1}{2}$ years (8 years for passing all six of the 8-year tests and $\frac{1}{6}$ of a year for each test passed above these, making the score 8 and $\frac{9}{6}$ or $9\frac{1}{2}$ years).

It is very unlikely, however, that the tests are accurately placed even with respect to order. Results of experiment show that the tests in each age group in the Binet scale are far from being at the same level. Allowing only for chance errors, we may assume that the six tests which are supposed to be 8-year tests are in reality distributed on the scale according to the law of distribution, and that the same is true of the other groups of tests. The positions of the six small circles (Fig. 2*C*) on the dotted curves represent the possible degrees of difficulty of the six tests of each group. If it be assumed first that the degree of ability in each function is at a level of 8-year intelligence, we see that the child can pass test *a* in group IX., tests *a*, *b*, and *c*, in group VIII., tests *a*, *b*, *c*, *d*, and *e*, in group VII., and all in group VI. He would therefore receive a score of 6 and $\frac{9}{6}$ or $7\frac{1}{2}$ years (again a half year too little). And if we assume the distribution of specific abilities to be "normal" as represented by the curve, of which the average, median, or mode is 8 years, we

get again a score of $7\frac{1}{2}$ (5 and $\frac{15}{6}$) by the same method as before.

If we assume that the tests were of such character as to test only two functions (*a* and *b* at *D*, Fig. 2), that the tests of each age-group were distributed at fairly equal intervals as shown, and that the degrees of the two functions measured were as indicated by the vertical lines, the child would then pass all the 5-year tests, four 6-year tests, two 7-year tests and no 8-year tests, giving a score of 6 years, while the average of the probable values of the two functions is seen to be $6\frac{1}{2}$ years. Again the score is a half year too low.

Finally, let us assume that each test measures a separate function (Fig. 2*E*; *a*, *b*, *c*, . . .), the height of the circle or cross measuring the degree of difficulty of the test in each function. Supposing those tests passed to be as indicated by the circles and those failed to be as indicated by the crosses, we should, for lack of more data concerning the degree of each function involved, be compelled to assume that it was approximately as indicated by the vertical lines. An average of the probable values of the functions obtained from significant tests would be $7\frac{1}{2}$ years, while the score that would be given under these circumstances is only 7 years. It is therefore quite certain that the method of scoring is in error in that a half year is subtracted from every score.

The Effect of the Error in Scoring.—It is this error which in all probability obscures the error of using the bases other than the 50 per cent. basis for standardizing tests, or has seemed to necessitate this counterbalancing error, as has been suggested. Kuhlmann says: "Taking other facts already brought out into consideration, it becomes evident further that this percentage should vary some with the age group. It should be higher for the lower age groups than for the higher age groups." It would seem to be the aim of Kuhlmann, Terman and others to find a series of per cents. above 50 per cent. such that the "ten-year tests" derived by the use of these per cents. will be easy enough so that when scored by the present method (which is erroneous in subtracting a half year from each true mental age) 50 per cent. of ten-year-

olds will be able to achieve a score of ten years in mental age. The necessity for such a series of per cents. may be obviated in either of two ways, in both of which the single 50 per cent. basis is used throughout. First, one half year may be added to each score as obtained at present, or, that which amounts to the same thing, we may proceed to score as at present except that a child is given a mental age of ten years if he passes tests equal in number to all up to and including *one half* of the ten-year group instead of all of them, etc.; or, second, if it be desired to score the tests precisely as at present, this may be done if those tests are placed in the ten-year group which are standard for ages ranging from nine to ten, thus averaging 9½ years, and if those tests are placed in the nine-year group which are standard for ages ranging from eight to nine, thus averaging 8½, etc. In using the second method it will be seen, as shown at *F* in Fig. 2, that if a child passes one half of the so-called ten-year group he will receive a score of 9½ years which will be correct, and if he passes all the tests of the so-called ten-year group he will receive a score of ten years which will be correct, etc. The only objection to this method is that those tests which are called the ten-year group are standard for ages averaging only 9½ years and hence are not correctly named. This is perhaps of no consequence if the facts are understood. In view of the fact also that it would be difficult to establish a new method of scoring, this latter method of constructing the scale is recommended.

A Consideration of Intelligence as a Function of Age

That ten-year intelligence is relatively closer to nine-year intelligence than nine-year intelligence is to eight-year intelligence is suggested by the fact that there are fewer ten-year-olds who exceed nine-year intelligence than nine-year-olds who exceed eight-year intelligence. And the fact that the number of 17-year-olds, 18-year-olds and 19-year-olds who exceed 16-year intelligence is practically the same as the number of 16-year-olds who exceed it suggests that while intelligence increases with age, the increments decrease with

age, and that after about the age of 16 years the intelligence does not increase at all, or but very little.

If we had curves representing the growth of intelligence in a number of children ranging from below normal to above normal, in which the years were measured as abscissas and the intelligences as ordinates, the curves would probably look somewhat like those in Fig. 3. The dots on the curves

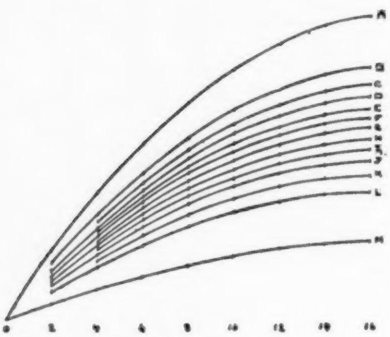

Fig. 3.

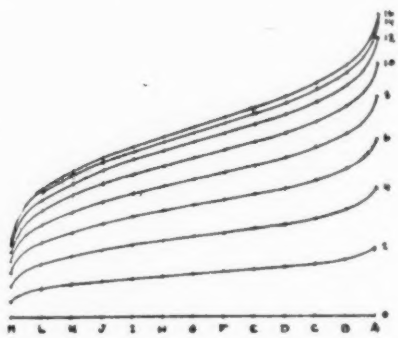

Fig. 4.

Fig. 3. Hypothetical curves of growth of intelligence for individuals of varying degrees of brightness.

Fig. 4. Hypothetical curves of distribution of intelligences at various ages (ogive form).

A, *B*, *C*, etc., represent the tops of the ordinates measuring the intelligences of the individuals for different years of their ages.

If on the other hand we were to consider the individuals placed in order of brightness at equal intervals along the horizontal axis and their intelligences in successive years dotted in one vertical column for each (as shown in the case of individuals *A*, *B*, *C*, etc., in Fig. 4), and if we then drew a smooth curve through the dots representing the intelligences of each at two years, four years, six years, etc., we would have a series of curves as shown in Fig. 4. These curves represent the distribution of mental ages for each chronological age.

The following will serve as an analogy. Suppose runners were arranged in order of ability from left to right and started

together. At the end of say two minutes they would be arranged in a curve similar to the first one above the horizontal axis. At the end of four minutes they would form a curve similar to the second, etc. The distance traversed by any runner would be slightly less during each succeeding minute. To complete the analogy we should suppose the rates of the different runners to be proportional throughout and that they came to a stop at the end of 16 minutes.[1]

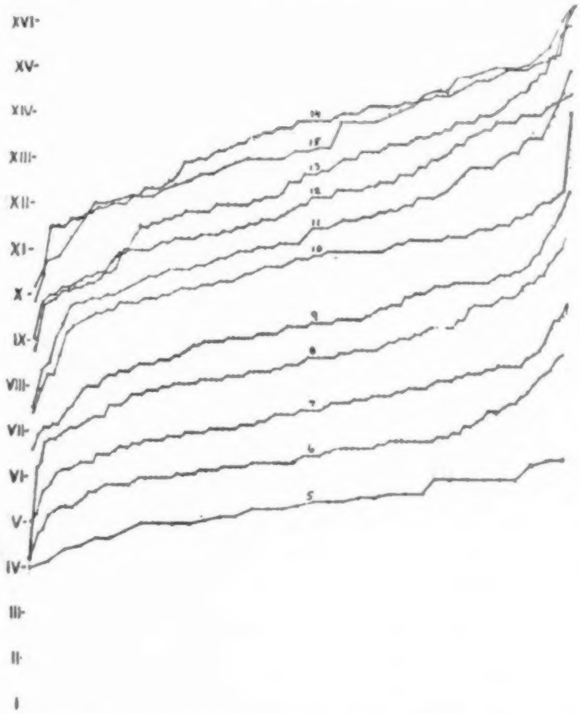

FIG. 5. Distribution of mental ages of a large number of children of various ages as found by Terman.

Curves analogous to these, representing the distribution of mental ages for each chronological age of a large group of elementary school children tested by Terman, are shown in

[1] The curves may be considered also as being made up of an infinite number of points, the heights of those in the same vertical line representing the different degrees of intelligence of a single individual at successive ages.

Fig. 5.[2] In this group the 15-year children appear slightly selected.

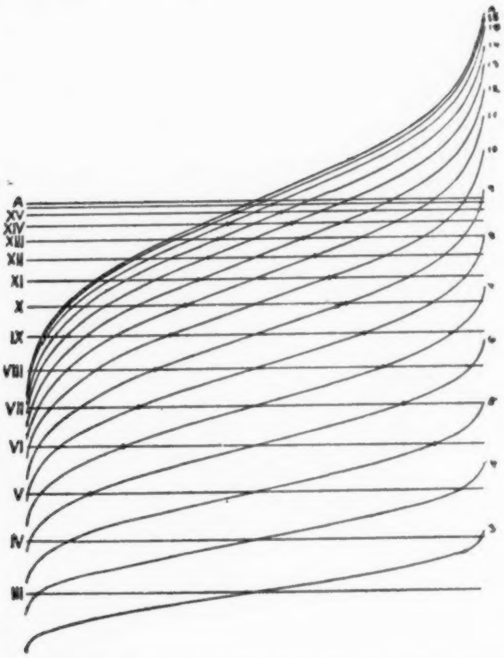

FIG. 6. Hypothetical curves of distribution of intelligence at the various ages showing the levels of intelligence which would be standard for the various ages and the portion of the distribution at each age (shown between the two dots) which would be considered "at-age."

In Fig. 6 we have a complete set of the curves of which those in Fig. 4 illustrate the derivation.[1] Horizontal lines

[2] The height of each dot above the horizontal axis measures the mental age of a single individual. As may be seen, they are arranged in the order of mental ages in each group of a single chronological age, at equal horizontal distances, and then joined by lines. Such a 'curve' or *ogive*, of course, gives more complete data than the histogram in which only the size and extent of groups are given by rectangles. The mental ages are computed on the basis of a tentative arrangement of the tests by Terman and not on his final revision of the Binet scale.

[1] The ogive form of the curve of distribution, of which the equation is $y = ke^{-h^2x^2}$, was chosen in preference to the common bell-shaped curve, of which the equation is $y = \int ke^{-h^2x^2}dx$ for the reason that it admits of greater ease of comparison of a number of curves in juxtaposition. It will be noted that the range of distribution in each curve has been made proportional to its distance above the horizontal axis, or, in other words, the coefficient of dispersion is the same for all the curves. This follows from Fig. 3

representing the standard intelligences at each age are drawn. A dot on the curve of the 10-year-olds, at a point where the curve crosses the 11-year level, marks the dividing line between those 10-year-olds who fall below and those who exceed 11-year intelligence. A dot on the 10-year curve at the 9-year level shows what portion pass the 9-year intelligence level. If we define as 'at age' those 10-year-olds whose intelligence falls between 9-year and the 11-year levels, that proportion of 10-year-olds will be represented by the proportion of horizontal distance between the two dots on the 10-year curve, and similarly for the curves of other years. We see at a glance that fewer are at age in the older groups than in the younger. This is true for two reasons, each self-sufficient: first, because the curves are closer together at the upper ages; and second, because the curves have a greater slant at the upper ages, which represents the fact that the distribution of a certain portion of a group is wider in the upper ages.

Consideration of the More Accurate Placing of Tests

In the building of a tentative scale, it is of course desirable to place each test as accurately as possible, conveniently, as to the age for which it shall be considered standard. This may be done by considering not merely the per cent. of children of one certain age which pass the test but by taking into account also the per cents. passing at several different ages. Thus, suppose the per cents. of children at the ages of 9, 10, and 11, passing a certain test were respectively 44 per cent., 50 per cent., and 74 per cent. By the consideration of the 50 per cent. alone we would immediately say that it was a 10-year test. Now let us suppose that we knew that the per cents. at these three ages which should pass a 10-year test were respectively 32 per cent., 50 per cent., and 63 per cent. Then, judging by the two per cents. other than the 50 per cent., we should be inclined to think that the

in which the assumption is made that the difference in intelligence between two persons of the same age, through successive years, increases in proportion to the increase in the absolute amounts of their intelligences.

test was really easier than a ten-year test and that the 50 per cent. had been accidentally too low. A knowledge of the per cents. of 9- and 11-year children which would normally be expected to pass a 10-year test would therefore be of value for the more accurate placing of the test, and of course, the same would hold for the other tests. Having fixed 50 per cent. as the per cent. of children which should pass each test at the age to which it is to be assigned, it is our problem to determine the per cents. of children which should pass any test at the age of one year more, one year less, and if possible, two years more and two years less than the age for which the test shall be considered standard, etc.

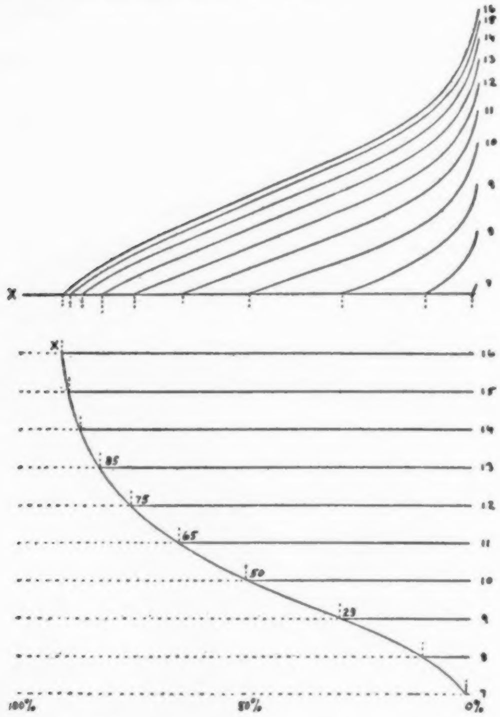

FIG. 7. Showing the derivation of a hypothetical "test curve."

If we examine the intersections at the horizontal line through the median value of the intelligences of the ten-year-olds (see Figs. 6 and 7), it will appear that 10-year intelligence

will be exceeded by perhaps 29 per cent. of 9-year-olds, 65 per cent. of 11-year-olds, 75 per cent. of 12-year-olds, 82 per cent. of 13-year-olds, etc. These would, therefore, be the hypothetical per cents. of children at these ages which would be expected to pass a 10-year test. It will be shown how values of these per cents. may be derived from data.

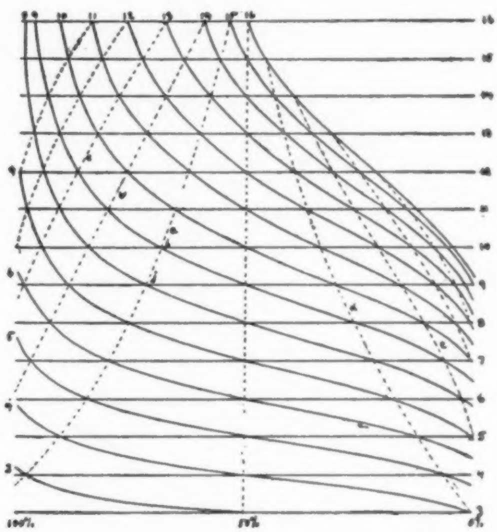

FIG. 8. A group of hypothetical test curves derived as in Fig. 5, showing that the intersections of the test curves and age lines lie in smooth curves.

If on horizontal lines representing the various ages (Fig. 7) we lay off the per cents. of children at these ages which are indicated by Fig. 6 to have intelligences above 10-year intelligence (these per cents. being represented as full lines in the figure),[1] we can draw a curve through these points. In Fig. 8 is shown the whole series of curves of which curve 10 is the same as the curve of Fig. 7. The intersection of each curve with the horizontal age lines marks the per cent. of children of those ages which are indicated by Fig. 6 to have intelligences above that standard for the age represented by the curve and which would be expected, therefore, to pass

[1] For reasons which will appear, it was thought best to measure these per cents. from the right rather than from the left.

a test standard for that age. Intersection (*i*) indicates the per cent. of 10-year-olds who would be expected to pass a 9-year test; intersection (*j*) indicates the per cent. of 9-year-olds who would be expected to pass an 8-year test, etc. It will be seen that the intersections indicating the per cents. of children at each age who would pass the test standard for the age of one year less, lie in a smooth curve (*a*). So do the intersections indicating the per cents. of children of the various ages which would pass a test standard for the age of two years less, etc. Now if we had the true position of these curves (*a*, *b*, etc.), we could draw the true test curves, giving the per cents. of children at the various ages who would pass a test standard for any age. If we had the per cents. of children of various ages which passed certain tests at the age of one year more than the age at which just 50 per cent. passed

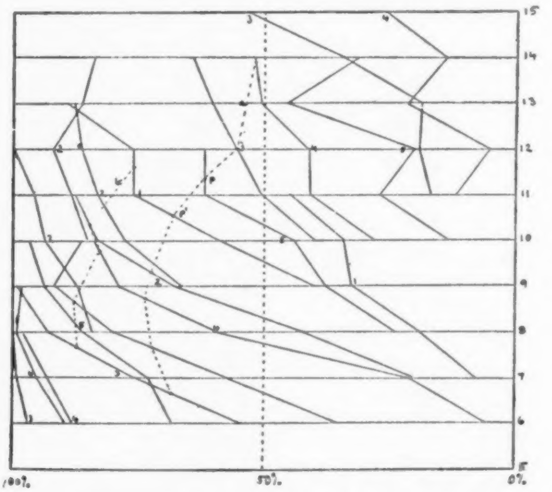

FIG. 9. Some test curves derived from data given by Kuhlmann showing the points *p*, *p′*, etc., which would indicate the per cents. of children by which it was assumed the tests would have been passed at the age of one year more than the age at which just 50 per cent. would have passed them.

them and plotted these per cents. on age lines as in Fig. 8, it is presumable that these would lie in a position approximating that of curve (*a*) in Fig. 8. A smooth curve drawn then through these points would give us an approximately correct position of this curve.

Kuhlmann[1] gives us per cents. of children at various ages which passed each of the tests of the scale. Certain of these per cents. were plotted in Fig. 9 and the points joined with broken lines analogous to the test curves in Fig. 8. The broken line in the center marked 5, for example, shows that test No. 5 in group X. was passed by 24 per cent. of 8-year-olds, 38 per cent. of 9-year-olds, 44 per cent. of 10-year-olds, 62 per cent. of 11-year-olds, and 62 per cent. of 12-year-olds. In order to make a rough estimate as to the age at which just 50 per cent. would have passed the test, we may note that the broken line crosses the 50 per cent. line at a point representing approximately the age of ten years and four months, and we might assume that this is the age sought. To find the per cent. that would pass the test at the age of one year more than that at which just 50 per cent. pass it, we should look to see at what point the broken line would cut the horizontal line corresponding to the age of eleven years and four months. This point (p) we find to correspond to approximately 62 per cent. The age at which 50 per cent. pass test No. 1 in group XI. is supposedly 9½ years, since the broken line crosses the 50 per cent. line at this point. The per cent. (p') which may be supposed to pass this test at the age of 10½ (one year more) is about 68 per cent. Similarly points corresponding to p and p' were found on the other broken lines and these are joined by the dotted line (a) which corresponds to dotted line (a) in Fig. 8. Similarly, dotted line (b) in Fig. 9 corresponds to dotted line (b) in Fig. 8.

The points on the dotted lines a and b in Fig. 9 were transferred to another plot (Fig. 10) and in addition to these were added dots similarly obtained from the plotting of all other per cents. given in Kuhlmann's table and a large number of per cents. found by Terman and others. The points designating the per cents. of children passing each test at the age of one year above or below that at which it was assumed just 50 per cent. would have passed it, are

[1] 'Results of Examining a Thousand Public School Children with a Revision of the Binet-Simon Tests of Intelligence by Untrained Examiners,' *J. of Psycho-Asthenics*, June, 1914.

indicated by dots; two years above or below by short horizontal lines; and three years above or below by short vertical lines. Smooth curves were then drawn through these groups

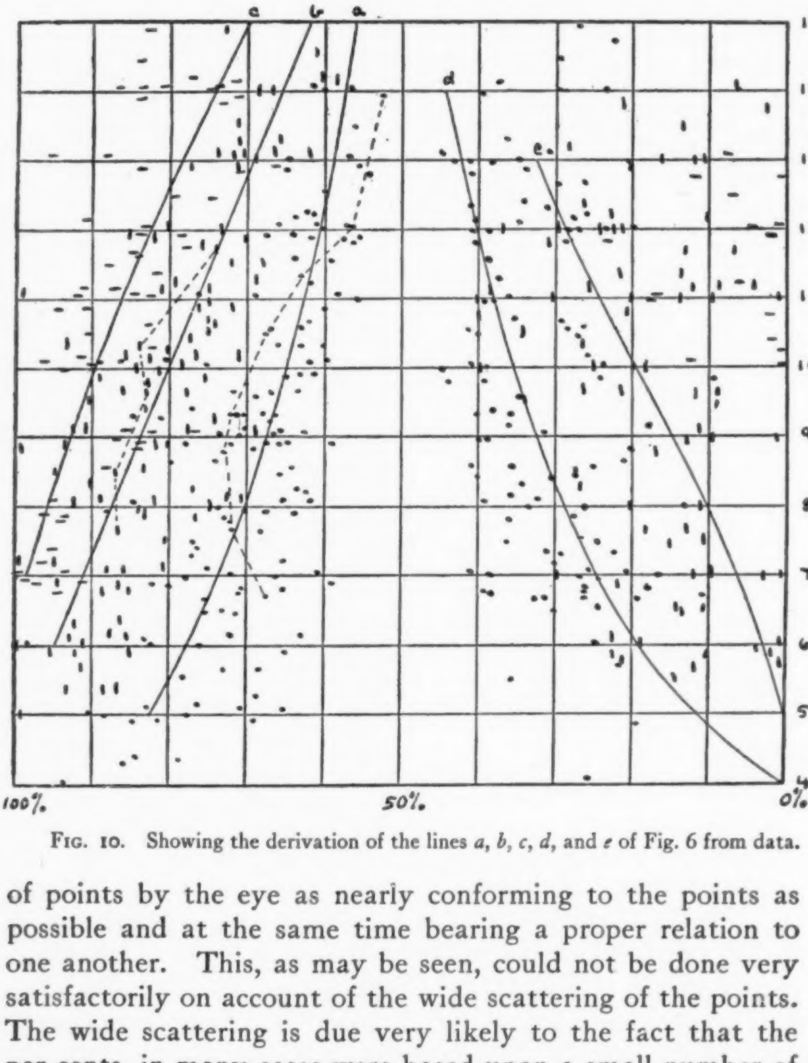

FIG. 10. Showing the derivation of the lines *a*, *b*, *c*, *d*, and *e* of Fig. 6 from data.

of points by the eye as nearly conforming to the points as possible and at the same time bearing a proper relation to one another. This, as may be seen, could not be done very satisfactorily on account of the wide scattering of the points. The wide scattering is due very likely to the fact that the per cents. in many cases were based upon a small number of children. The curves *a*, *b*, *c*, *d*, and *e* in Fig. 10 correspond to curves *a*, *b*, *c*, *d*, and *e* in Fig. 9, the former being hypothetical and the latter derived from data. It will be con-

ceded, I think, that the data are of such character as to support the hypothesis upon which curves (*a*, *b*, *c*, *d*, and *e*) were derived.

The curves *a*, *b*, *c*, *d*, and *e* in Fig. 10 were then transferred to another plot (Fig. 11) and the generalized test curves drawn in. Since the 10-year test curve, for example, crosses the 9-year line at a point representing 32 per cent., we must regard this as the per cent. of 9-year-olds that we should expect to find passing a 10-year test, ideally. Simi-

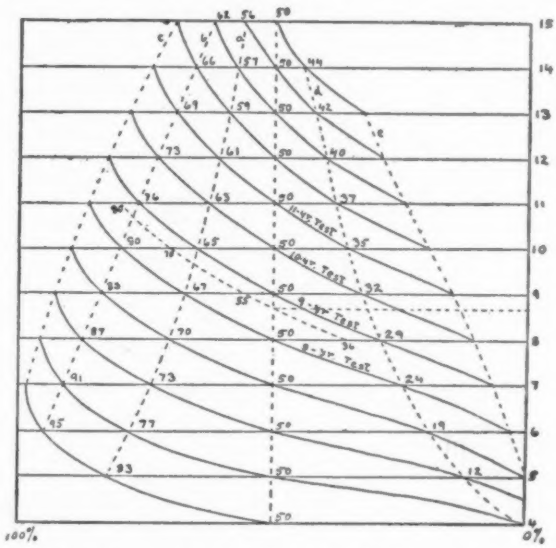

FIG. 11. True test curves as obtained from data.

larly we should expect to find 63 per cent. of 11-year-olds, 73 per cent. of 12-year-olds, etc., passing a ten-year test. It might be said that, as a corollary to the 50 per cent. basis of standardization of tests, a test passed by 32 per cent. of 9-year-olds is a ten-year test, or a test passed by 63 per cent. of 11-year-olds is a ten-year test, etc. A dotted test curve in Fig. 11 crossing the 50 per cent. line at a point representing an age of 8⅔ years is found to cut the horizontal lines at points indicating that a test which would be standard for the age of 8⅔ years will be passed by 36 per cent. of 8-year-olds, 55 per cent. of 9-year-olds, 70 per cent. of 10-year-olds, 80 per cent. of 11-year-olds, etc.

What we desire to know, however, is the age for which a test should be considered standard if passed by *any* per cent. of children at any integral age. For this purpose Fig. 12

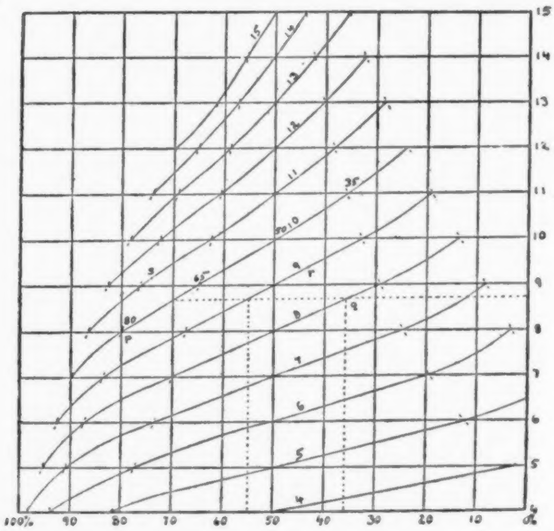

FIG. 12. Diagram for standardizing tests when the per cent. of children passing it at a certain age is known.

was constructed from the data of Fig. 11 in such a way that the curves in Fig. 12 correspond to the horizontal lines in Fig. 11 and vice versa. To find the age for which a test is standard which is passed by 80 per cent. of 10-year-olds, we note that the curve numbered 10 cuts the 80 per cent. vertical line at a point p opposite a height marked 8 on the vertical scale, showing that the test in question is an 8-year test. To find the age for which a test is standard which is passed by 36 per cent. of 8-year-olds, we find the point q at which curve 8 (representing 8-year-olds) cuts a vertical line corresponding to 36 per cent. (shown dotted) and note that this point is opposite a height on the vertical scale 8⅔ years; therefore the test in question is standard for that age. A test passed by 55 per cent. of 9-year-olds will be seen by the same figure to be standard also for the age of 8⅔ years. These facts correspond to those noted concerning the dotted

curve in Fig. 11. In this way the age for which a test is standard may be found for *any* per cent. of children, not too far from 50 per cent., passing it at any age.

Returning now to the consideration of the test passed by 44 per cent., 50 per cent., and 74 per cent. respectively of 9-, 10-, and 11-year children, we find by examining Fig. 12 that the passing of the test by 44 per cent. of 9-year-olds would indicate (see point *r*) that it is standard for the age of about 9⅓ years, and the passing of the test by 74 per cent. of 11-year children would indicate (see point *s*) that it is standard for the age of about 9⅙ years. Averaging 10, 9⅓ and 9⅙ we get 9½ years as the age which is probably more nearly correct than any of the others, as the age for which the test is standard.

Another method of combining the indications of three per cents. is to add them and find the indication of the sum. For example, the sum of the per cents. of children at the ages of 8, 9, and 10 who are expected to pass a 9-year test by Fig. 11 is 144 (29, 50 and 65). Since the sum of the three per cents. at these three ages passing an easier test is greater than 144 and the sum of the per cents. at these three ages passing a more difficult test is less than 144, therefore if the sum of the per cents. of children at the ages of 8, 9, and 10 is 144, we know it is a 9-year test. Similarly, if the sum of the per cents. of children at the ages of 8, 9 and 10 passing a test is 161 (36, 55, and 70, see Fig. 11) we should consider the test standard for the age of 8⅓ years.

In order to find the age for which a test should be considered standard when passed by three per cents. at any three consecutive ages of which the sum is *any* number, a diagram (Fig. 13) was constructed. It will be noted that the slanting line labeled 8–9–10 crosses the vertical line 9 at a height opposite 144 on the vertical scale. This indicates that that test is a 9-year test if the sum of the per cents. of children passing it at the ages of 8, 9, and 10 is 144 (29, 50 and 63). To find the age for which a test should be considered standard if the sum of the per cents. of children passing it at the ages of 8, 9 and 10 is 161 (36, 55 and 70) we note

that the slanting line 8–9–10 attains a height equal to 161 in the vertical scale at a point *p*, denoting that the test is standard for the age of 8⅔ years, which corresponds to the age at which 50 per cent. would be expected to pass it, as

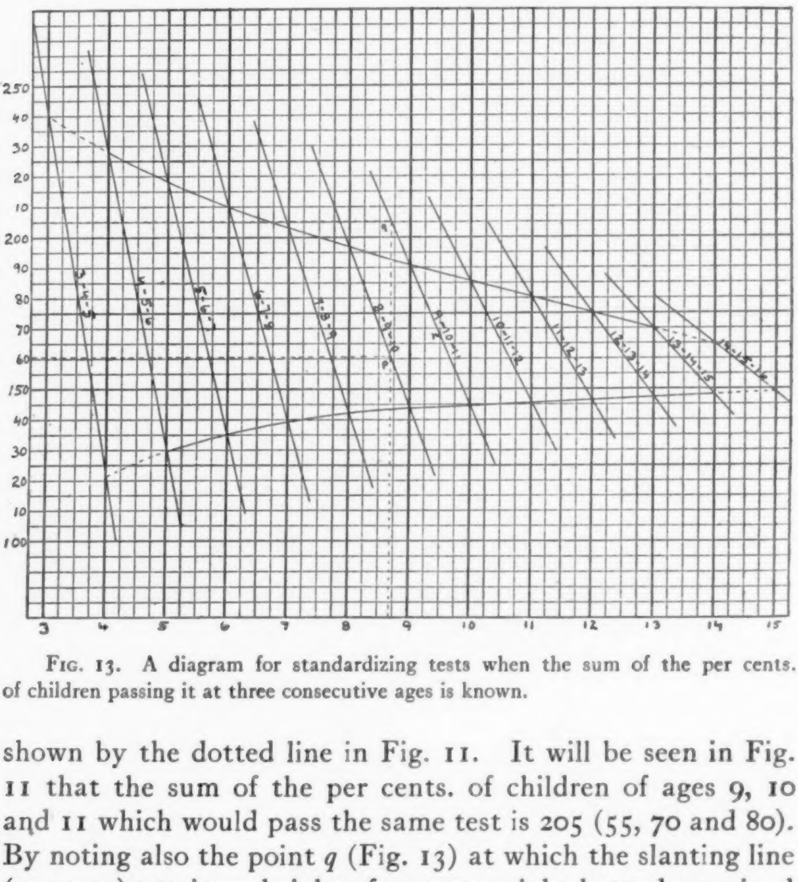

FIG. 13. A diagram for standardizing tests when the sum of the per cents. of children passing it at three consecutive ages is known.

shown by the dotted line in Fig. 11. It will be seen in Fig. 11 that the sum of the per cents. of children of ages 9, 10 and 11 which would pass the same test is 205 (55, 70 and 80). By noting also the point *q* (Fig. 13) at which the slanting line (9–10–11) attains a height of 205, we might have determined the age, 8⅔ years, for which the test is standard.

Returning to the test passed by 44 per cent., 50 per cent. and 74 per cent., respectively, of children of 9, 10 and 11 years, the sum of the per cents. being 168, we may note the point *r* in Fig. 13 at which the slanting line 9–10–11 attains a height of 168 and find that it is opposite a point on the horizontal scale which would be 9½, denoting that the test

should be considered standard for the age of 9½. This is what we found by taking the averages of the ages of standardization found singly from the three per cents. by the former method. We therefore have two methods by which a test may be placed by the consideration of at least three per cents. By the former method, of course, as many per cents. may be utilized as are available, thus making possible in some cases the combining of the results of several different investigators. It was thought best not to attempt a diagram for the sum of more than three per cents. since the data do not seem sufficiently definite in the matter of per cents. passing a test at the ages of two years below and three years above the age for which it was estimated that 50 per cent. would have passed it. Furthermore, as will be shown later, if the three central per cents. do not sufficiently conform to the curves in Fig. 11 the test is not a proper one to use.

Fig. 14 shows the distribution of the tests used by Kuhlmann in obtaining certain per cents. which he has found.[1]

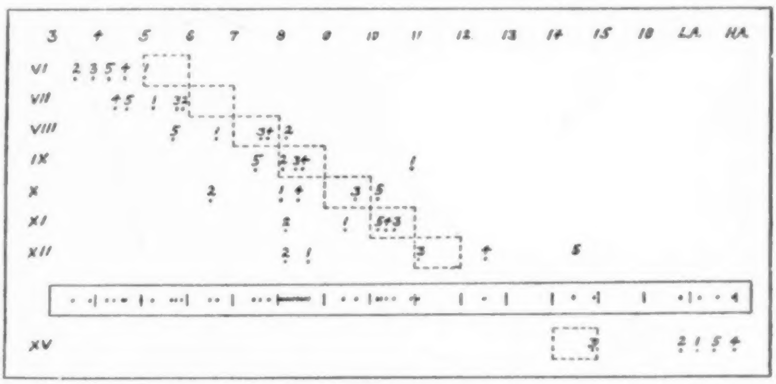

Fig. 14. Showing the distribution of 40 tests according to per cents. of children passing them obtained by Kuhlmann.

These tests were placed by means of the sum of the three per cents. nearest 50 per cent. and the use of the diagram in Fig. 13 wherever possible. In certain cases in which all the per cents. lay above or below 50 per cent., it was necessary

[1] F. Kuhlmann, 'Some Results of Examining a Thousand Public School Children with a Revision of the Binet-Simon Tests of Intelligence by Untrained Examiners,' *J. of Psycho-Asthenics*, June, 1914, p. 255.

to make an estimate by the average method, using Fig. 12. The per cents. in the cases of most of the tests in groups VI. and XV. were so far from 50 per cent. that only a rough estimate could be made. The figures 2, 1, 4, 3, and 5 opposite the number X. at the left, for example, indicate that in group X., test No. 2 was in reality standard for about the age of 6.5 years, test No. 1 for about the age of 8 years, No. 4 for about the age of 8.4 years, No. 3 for about the age of 9.6 years, and No. 5 for about the age of 10.2 years. If the tests are to be scored so that a child passing all the '10-year' tests, for example, and no others, is accorded a mental age of ten years, then those tests must be placed in the 10-year group which are standard for ages averaging 9½ years. Since this is believed to be the method used in deriving mental ages from these tests, they should have been standard for ages falling within the small dotted rectangles. While the placing of the tests could have been done more accurately if more data had been available, it is obvious that many of the tests are far from being properly placed. The distribution of all the tests taken as a scale is shown in the long rectangle.

It will be noted that the overlapping of the distribution of one age group with that of another is partially compensating, so that the scale as a whole is not as far from accurate as the individual groups. In fact, it is not the overlapping at all which vitiates the scale but it is the irregular spacing of the tests. It is, of course, of no great consequence how the tests are grouped for scoring on the testing blank, but it is of great importance to know that they are more accurately spaced on the true scale than is the case in Fig. 14. The presence of too many tests between the ages of 8 and 9 on the scale operates to make a break in the age distributions such as that shown between the 9-year-olds and the 10-year-olds in Fig. 5.[1] In a case of this kind, tests where too thickly grouped should be either altered or replaced by tests which would fill in where sparse. Later will be discussed the need of variability in the spacing of the tests.

(To be continued)

[1] Most of this error has been corrected in the Stanford Revision of the Binet Scale.

COMPLETENESS OF RESPONSE AS AN EXPLANATION PRINCIPLE IN LEARNING

BY JOSEPH PETERSON

University of Minnesota

Though in more or less agreement with the recent discussions on the subject of selection principles in learning by Carr[1] and Watson,[2] the writer is of the opinion that an important principle or consideration is omitted in both discussions. It is to be noted that neither of these writers has made any claim to an attempt at completeness in his suggested explanations. Watson omits the factor of intensity and associates 'the process of substitution' with recency and repetition, though in a manner that hardly makes it coördinate with the latter factors.[3] He also mentions, without making further use of them, certain other factors, all of which are included with substitution in the principle here suggested by the writer. "Unquestionably," Watson says, "the principles of reënforcement, inhibition, and summation of stimuli are constantly operative. When the separate stages of habit have been more carefully analyzed we can more readily see how such factors operate in detail."[4]

Some years ago the writer attempted an explanation (unpublished) of the 'stamping-in-process' of successful acts on the principle of frequency in about the manner suggested by Stevenson Smith,[5] whose article he had not at the time seen. While the frequency factor, or principle, cannot cover every case of learning, and in itself is probably insufficient in any one case, its *modus operandi* has been indicated in a

[1] Carr, H. A., 'Principles of Selection in Animal Learning,' PSYCH. REV., 1914, XXI.

[2] Watson, J. B., 'Behavior: An Introduction to Comparative Psychology,' 1914, Ch. 7.

[3] *Ibid.*, pp. 272 ff.

[4] *Ibid.*, pp. 275, 276.

[5] *Jour. Comp. Neur. and Psych.*, 1908, 18.

general way by Carr and Watson. Professor Meyer has attempted a mechanical statement[1] of what may conceivably go on in the nervous system in the learning act.

The value of the factors of recency and (especially) intensity can be adequately stated, it appears to the writer, only in the case of the joint action of a number of tracts; not in terms of any one single neural tract. There is no question that many of our attempted neural explanations involving one arc, or at best a few neural arcs, are altogether too simple adequately to explain in such physical terms as we desire how one act can survive over the other more or less random acts because of its greater success in meeting the needs of the organism. Even in the simplest act involving so-called conscious control numerous neural tracts are called into play, varying in degree of directness or indirectness of connection with the muscles immediately concerned in the act. Professor Meyer's explanations are suggestive in this regard, but not of much use as they are based on analogies chosen from mechanical structures which make experimental application to behavior phenomena difficult. In a complex condition such as we actually find in the nervous and muscular systems, where various more or less related acts are involved in each reaction, some of these acts may be of an inhibitory nature to others under certain circumstances, while occasionally under other conditions all may tend rather positively to aid or strengthen one another. These mutually inhibiting or reinforcing effects would be determined not only by the nature and complexity of the stimulus but also by the inherited and acquired disposition—neural connections, bodily structure, etc.—of the organism. I shall refer to these mutually reinforcing and mutually inhibiting functions, in all the degrees between these two extremes, as the *principle of completeness of response.*

In certain passages Dr. Carr, in the article referred to, seems to have come close to this principle. *E. g.*: "From the standpoint of the immediate sensori-motor situation in which the animal is placed, the true path and the cul de sacs

[1] Meyer, Max, 'Fundamental Laws of Human Behavior,' 1911.

are to be distinguished from each other on the basis of the degree to which they impede or encourage the animal's activity. A blind alley . . . means hesitation, caution, investigation, or disastrous sensory consequences. The true path presents fewer obstacles; it offers greater encouragement to freedom, continuity, rapidity, and vigor of motor expression.[1] The difference is merely one of *degree*. The blinds check, thwart, and suppress activity more than does the true path, while the latter encourages and facilitates activity more than does a blind alley. The principle of relative intensity is here effective; acts are selected or eliminated according to whether the sensory consequences tend to facilitate and intensify them on the one hand, or to disrupt and suppress them on the other."[2] Yet this suppression or facilitation is here in no way explained by the principle of relative intensity; relative intensity, as well as the suppression and encouragement, of certain of the attempted acts is rather a consequence of a sort of cumulative attitude, or incomplete activity brought about by the overlapping of partially complete responses. Again, "The animal does not react to this complex situation as a unitary whole, as a single stimulus. He reacts to it selectively, and as a series of stimuli. There is a circular interaction between the sensory stimuli and the animal's movements. Each act modifies the stimulus in some respect, and the change of stimulus in turn modifies the act."[3] Here Dr. Carr recognizes the need of the complex situation affecting the response, though the nature of the selection is not made clear.

In the case of the maze problem the animal on entering a cul de sac—or any other path, in fact—responds at first more or less incompletely, *because all the subordinate activities involved cannot take place at once.* If the animal's progress is soon checked in a blind alley the animal is not seriously nonplused. Certain elements of the general response are tending to drain into other alleys that may recently have been passed,

[1] This is of course true not for any momentary status of the animal but only for a larger situation involving successive stages of acts, or series of acts.

[2] *Op. cit.*, p. 162.

[3] *Ibid.*, p. 157.

thus partially dividing the animal's activity. These elements now prevail when the others are checked. Let us suppose that the correct path, *A*, has just been passed when the animal suddenly comes to the end of the cul-de-sac, *B*. The tendencies to respond to *A* are still surviving and now direct the impeded activity into this, the successful, path. If, on the other hand, the correct path had been chosen the first time the distracting impulses toward *B* would have become fainter and fainter as the animal proceeded into *A*, and would finally have faded away. The principle is not different when the complexity of the situation is increased. When the food is finally reached all the remaining delayed reactions, the tendencies, still persisting, to go into other alleys recently passed, are relaxed—the act as a whole is complete.

Thus by an actual overlapping of many tendencies to respond in diverse ways the erroneous tendencies are directed into the successful ones, and the latter are strengthened by reinforcement. Without such overlapping of various impulses in the same general response, the inhibiting effects of the successful upon the unsuccessful or irrelevant tendencies are incomprehensible. How can a successful result act backwards and strengthen the impulses leading up to it and stamp out the unsuccessful impulses? It is a mistake to look upon these tendencies as separate *acts* each complete in itself and occupying the whole arena for the time being. This seems to make clear why the pleasurable act survives over the other acts: the pleasure itself is not a cause or natural antecedent of the surviving act, but only the inner or 'felt' aspect of it and therefore valueless in explanations, though no less a fact to the individual performing the act. If analogies help us in conceiving this selective process we can find very good ones in a stream of water making its way initially over an uneven and loose soil. Now the water plunges mainly into this little hollow place drawing noticeably upon neighboring portions of the stream; now, this place being filled (cul de sac), the principal part of the current passes on to fill some other depression into which a small overflow had

already begun but was impeded by the main plunge of the stream into the preceding hollow.

The selectiveness, then, is due finally to the entire conformation of the organism together with the present more or less complex stimulating conditions; more immediately it is due to the cumulative effect of various incomplete partial-responses. This is admittedly a rather complicated matter to introduce, too complex adequately to state in terms of simple nerve tracts. Yet without considering the whole situation *together* such terms as free or impeded activity can hardly mean anything. The selectiveness of the organism is simply its more easy adaptation to certain direct and indirect stimuli than to others; but worked out in detail this is not a simple matter. In the case of impeded activity there are more internal conflicts—conflicts among elementary neural and muscular processes than in unimpeded activity. The latter type of response is more *complete*, or unitary, than the other. That responses are always more or less complex is a fact that is not fully enough considered in our usual simple neural explanations. And more complex explanations in terms of nerve impulses are extremely difficult, because of insufficient knowledge on many points of importance. The usual statement is that a lessened resistance is formed along certain tracts due to repetition, or recency, or intensity, or to the combined action of any two or all three of them. Intensity (of what?—stimulus or response? or both?) obviously implies on the motor side a harmonious action of a system of tracts and of various muscular responses, a mutual reinforcement; while on the sensory side it may mean a more effective stimulus for such harmonious activity, not merely physical intensity. The latter condition is illustrated in the heightened effect of a very weak stimulus that is intensely interesting, such as a moving object. The interest is obviously due to some bodily organization, inherited (instincts) or acquired (associations), making certain kinds of responses complete and others considerably impeded on account of the inhibitory, or mutually blocking, action of the constituent elements. The pleasurable tone which accompanies certain

of our acts is of course only a subjective indication that the response is along the line of least resistance. This is true only up to a certain limit at which the act approaches a neutrally toned reflex. We are coming to the point now in psychology at which we cannot look upon states of feeling as *causes* of action. The same is true, of course, of 'ideas.'

The neural correlates of learning processes could not be stated in terms of changes along any particular tract even if it were conceivable that certain of the 'controlled acts' involve but a single nerve tract. Such processes really involve more or less complex *attitudes*, and light is thrown upon them by the delayed reaction experiments of Hunter and others. On account of the complexity of stimulating conditions—some stimuli being direct, others indirect by means of association, and all varying enormously in their degrees of intensity—the various elementary movements involved are doubtless always more or less in conflict; *i. e.*, the total reaction is in a degree incomplete, tentative. It is conditioned by various muscular 'sets,' or tensions, partial responses to immediately distracting stimuli, which cannot relax wholly until relief is obtained from confinement, or food is reached; and even then they likely fade away gradually, if we may trust introspection of our own attitudes. If Dr. Watson will permit a bit of anthropomorphism—a fault of which his recent book is not, by the way, wholly spotless!—this hesitant, delayed, or incomplete response is such as a person may experience in relation to the position of the head if he has just passed some mischievous lad preparing to throw a snowball at him. The attitude may disappear rather soon after the ball has whizzed by the ears; it fades slowly if the ball does not come at all until one is out of reach. This incompleteness in reaction need not imply the sort of imagery which many comparative psychologists are desirous of keeping out of explanations in animal behavior. It involves nothing more in this regard than do the delayed reactions which Watson attempts to explain on the basis of continuous bodily orientation, together with minor muscular tensions and neural inhibitions and reinforcements not observable externally.

These neural processes *may* of course in the case of man, and possibly also in the case of some of the higher animals, be accompanied by the consciousness of images. The writer is convinced that his own auditory and visual 'imagery' is mainly, or almost if not quite entirely, a matter of finer muscular adjustments and motor tendencies of body, head, eyes, and certain ear structures. He is not inclined, however, to deny that certain persons may be able to call into play a richer supply of brightness, color, and tone qualities to supplement these muscular tensions though dependent upon them. But, again, *images are not consistently regarded as causes;* they are the inner aspects of certain results of direct or indirect stimulation.

The solving of any problem, such as the inclined plane, involves numerous synchronous more or less subordinate responses, as well as a complexity of stimulating conditions. Some of these responses are certainly much more immediate and direct than are others. There is therefore a continuous overlapping of responses, some of which are in opposition while others are mutually helpful and serve to the main response as additional stimuli, the latter leading to a more easy and complete expression. In our observation of animal behavior we have been too much interested in the principal response of the animal and have neglected to note sufficiently all the subordinate attitudes and responses. This is to be expected in so new a science, one, moreover, in which we are obliged to resort to objective methods. The inadequacy of the usual procedure comes out more conspicuously, it must also be noted, only when the nicer questions of explanation of the learning process arise.

Certain acts on the whole are chosen, to recapitulate in part, not because they are pleasant but because they are on the whole the most natural. A wrong view on this point leads to serious complications in the explanation and control of behavior, and in the field of human conduct it has done much toward making the study of ethics a mere academic affair and in many of its stages and aspects surprisingly unscientific. That the more complete reactions, those finally

'chosen,' are the pleasant ones in the main is simply an indication of a subjective kind that the response is relatively complete, unimpeded, that it is harmonious with one's inherited and acquired organization; pleasantness, however much a fact—and a desirable fact—it may be in our lives, is not a serviceable explanation principle. This fact is made patent by two millenniums of ethical discussion, and comes out more strikingly in the simpler experimentally controlled behavior of lower animals. Since adjustment is hardly ever quite complete or at an end with any particular act, there may with rather short-ranged or temporary successes be momentarily increased activity and therefore a pleasant tone, only to be checked later. Such a procedure is evident even in a more nearly homogeneous mechanical process like a liquid flowing over an uneven surface.[1]

The recognition of this principle of completeness of response, or the overlapping of responses, makes more comprehensible than heretofore results obtained by certain experimenters, showing that an animal which has learned certain problems adapts itself to others—*i. e.*, learns them—more readily than one which is wholly untrained. In the former experiences the animal has habituated itself somewhat to the incomplete or tentative attitude so that it can more readily remedy its errors, or find the right response. Among themselves animals differ widely, as do also persons, in the degree to which they throw themselves whole-heartedly into each possible outlet that presents itself in a difficulty. Experience has important effects in modifying this general aspect of behavior. The general unnaturalness of apparatus to an animal is sufficient readily to give the animal this tentative attitude even though the new problem requires a response considerably different from those on which it has been trained. Of course in cases of this kind various factors enter, all of which are not reducible to any one principle.

In the case of the inclined plane problem it is noticeable

[1] Whatever is ultimately true—if ultimate in this sense has any meaning at all—as to the 'freedom of the will,' scientific explanation seems to be most successful in the field of behavior, as in other fields, when it proceeds on the assumption of determinism. Consider, *e. g.*, certain recent progress in applied psychology and sociology.

that rats proceed to the plane, after failure to get into the food, with considerable tension toward the door of the food box. Frequently at a certain stage in the learning the animal comes to a standstill, and then scurries back in vain to the door. The uncritical would say that the rat was trying to recall which way it went to success before. Since it must invariably push the inclined plane before entrance to the food can be obtained it is forced in every case to return to the plane, but it does this as if an elastic cord were constantly pulling it toward the food. These muscular tensions are released only when the proper reactions have made entrance to the food possible and when the food has been reached. In this way all the relevant acts are associated effectively together by what is tantamount to *simultaneous action*. The apparent backward effect of certain acts is thus made comprehensible. Even in cases when the animal goes directly to the inclined plane, if the habit is not fully established, it frequently shows hesitation and the effects of impulses toward the food box. The writer has frequently noted this hesitant behavior, and Dr. Florence Richardson's monograph supplies many instances.[1]

Applying this principle of overlapping of incomplete responses to a specific instance we find it of considerable help. Take the case of the choice of the correct alley as against the cul de sac discussed by Watson[2] on page 267. The figure is here reproduced. If the animal goes in the direction indicated by the arrow in *A*, it is not simply a matter of probability as to whether it will finally establish the habit of turning into *B* —for this probability argument gets very flimsy when you take into consideration a number of successive culs de sac. As the animal passes *B* on its way to *x* there are impulses to enter *B*, but they are outweighed by those tending

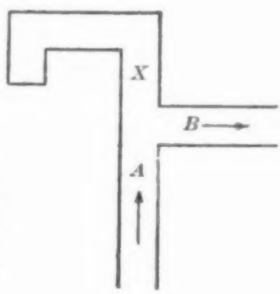

Fig. 1.

[1] 'A Study of Sensory Control in the Rat,' *Psych. Rev. Mon.*, 1909, Whole No. 48.
[2] 'Behavior.'

into *X*. As it returns from *X* these impulses, still carried over to an extent and therefore still effective, are potent toward directing it into *B*. Besides this fact is another one, based on the same principle: the attitude resulting from the general direction of the animal's moving, so long as it has not retraced its steps except in a blind alley, is operating against its returning into *A* in our figure. A study of illusions usually called being 'turned around,' to be published later, has convinced the writer that we are yet too neglectful of these larger attitudes in our studies of behavior. The correctness in general of this idea of the overlapping of responses in learning as a principle of explanation seems to be supported by the fact that a short cul de sac is less confusing than a long one, even though neither of them has any turns. This is true of very short ones; how far it will hold true in general is a matter yet to be determined, one the working out of which will make an interesting modification of the delayed reaction experiment.

A CASE OF PSEUDO-PROPHESY

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At the time of the great earthquake of 1906 the accompanying poster of the Junior Farce PKWTNOPIU of the Class of 1903 which was written by Harry Johnson and Fletcher Wagner, was referred to as a prophesy by some of the papers of San Francisco. As it seemed of interest to ascertain whether this was actually the case I wrote to Mr. Johnson, a consulting geologist in Los Angeles, the maker of the poster, in regard to the matter. His letter which I append shows that the contents of the poster[2] grew out of the drawing of an inference regarding the future from the past—is a case, in short, of scientific prediction.

Mr. Johnson says in his letter of Aug. 11, 1913:

"Mr. Fletcher Wagner, whom you may remember as winner in the Carnot Debate several years ago, first suggested to me the idea of a class play which should be like the famous breakfast food, 'something different.' At that time I was full of the geologic phenomena associated with the earthquake rift which passes southeastward along the San Francisco peninsula, back of Los Gatos, and so toward the Pajaro River. I have seen the effects of crustal movement during past ages along this fault line and have been deeply impressed with the topographic changes which have taken place in this part of California within comparatively recent time (geologically speaking). All of this was, of course, as you know, before the earthquake of 1906, but I realized that the fault line had been the theater of earthquake activities on a grand scale for a tremendous period of time.

[1] Read at the joint meeting of Section H of the American Association for the Advancement of Science and the American Psychological Association, San Francisco, August 3, 4, and 5, 1915.

[2] See the plate which gives a reproduction of the poster itself and of a photograph of Memorial Arch as it appeared just after the earthquake.

"What better than that Mr. Wagner, with his eye for the dramatic, should see in this geologic fairy tale a chance for a play that ought to appeal to at least Dr. Branner's students. Hence we sate ourselves down and began this immortal work. Fletcher composed the music, words, songs and pretty much everything else, so my creative outburst expressed itself, so to speak, volcanically. I knew that the earthquake rift ran northwest and southeast and felt that any high structures near it would probably be toppled over in case of an earthquake. The highest structure at the university, except the chimney, was the arch, and that seemed to lend itself best to a poster. I wanted the poster to be graphic, rather smashing in its effects, if you will, and so pulled out one side of it and left the arch overhanging in an impossible manner (I hope the engineering profession will have forgiven me this by now). Otherwise I tried to show what I thought would really happen in case the fault line had a chill. So far as any premonitions were concerned I know there were none. I merely drew a poster as best I could on the evidence of geologic facts gathered in the field. As I recollect, the poster was discussed considerably at the time of the earthquake in some of the San Francisco newspapers and of course, the usual vivid imagination of our newspaper friends called forth a long tale of the astounding clairvoyance of a Stanford student. Bosh and rot. You now have the real inwardness of this remarkable event put on paper for the first time."